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ART. I. *Remarks on certain Injuries of the Bones in Children.*—By JOHN RHEA BARTON, M. D. one of the Surgeons of the Philadelphia Alms-House Infirmary.

MUCH attention has been paid by surgeons to the treatment of fractures of the extremities; but their observations appear to have been confined principally to them as they occur in adults; scarcely noticing the peculiar injuries to which the bones of children are subject. We have certain rules laid down for the treatment of fractures in general, but little or no notice has been taken of a species of injury peculiar to infants, demanding also the same kind of management as fractures, though unaccompanied by two of their most prominent symptoms, viz. crepitus and displacement of fragments. The injury alluded to, is a simple *bending* of the bone, *occasioned by external violence.* This particular kind of accident has been noticed in a transient manner by Boyer, Aitken, Underwood, and others, though they have neither given us directions for the treatment of it, nor even described the nature of the injury.

There occasionally happens, perhaps more frequently than we are aware of, this intermediate kind of injury, between a simple contusion of the soft parts and a complete fracture of the bone.

Not unfrequently young children are brought to us labouring under an injury of the fore-arm, which after a very superficial examination, or perhaps from the unnatural shape of the arm alone, we pronounce to be a fracture, and treat the limb accordingly; but we do not invariably err upon this the safer side; on the contrary, since we are unable to produce crepitus in some cases, or feel the asperities of a fracture, we are apt to consider the bone to be uninjured, and attribute this deformity to tumefaction from simple contusion of the soft parts. Applications therefore are made with a view of discussing this swelling; but we find, perhaps when our mistake is irremediable, that this deformity depended upon an injury of the bone. If we examine such a case more attentively and enquire minutely into the symptoms, we shall find the arm painful, and deformed by a *curvature*, and the child deprived of its use; yet there is no derangement of fragments, nor can any crepitus be perceived, though there is motion at the part. If the radius and ulna be held at their extremities and an attempt made to bend it further, the *curvature* will be increased in proportion to the exertion used. If sufficient force be employed in the opposite direction the deformity will disappear, and the limb will resume its natural shape; but an attempt to produce the prominence on the other side, by continuing the pressure, would subject the arm to fracture. If the limb be again liberated the curvature will to a certain extent return.

The first three children in whom I had an opportunity of noticing this *bending* of the bones were all under the age of two years. I have since known it to occur in children further advanced. The first in which this peculiarity was observed, was in the infant of a poor woman in the neighbourhood of the Pennsylvania Hospital. The child had been placed upon some elevated spot, from which it fell to the floor. The mother perceiving it much injured, brought the child to the hospital immediately to be examined. It appeared to have fallen on the right arm, which was as before described, painful, useless, and deformed by a *curvature* on the inner or palmar side, but no asperity from a projecting fragment could be perceived, nor even the slightest crepitus felt; during the examination, however, the bones seemed disposed to yield, and on en-

couraging them by a little pressure the arm became very much curved. On attempting to press the radius and ulna toward each other, to observe whether a depression could be made anteriorly, both bones made firm resistance. When the pressure was employed to straighten the bones, it was effectual until they had resumed their natural shape, but additional force could not produce the prominence on the outer side: they would yield in the direction to which the accident had inclined them, but in no other. When the limb thus straightened was again liberated, the deformity would return spontaneously, but by no means to so great an extent as appeared immediately after the accident. The sensation conveyed in bringing the arm to its natural position, was somewhat similar to that which we experience in bending a piece of lead. Frequent attempts by such means as are commonly employed to detect crepitus were resorted to, but without effect. The case was so strongly marked as to leave no doubt as to the nature of the injury. Had it been a fracture with an interlocking of the fragments, it is true crepitus might not have been detected; but on attempting to bend it, instead of producing a gradual curve of the limb, there would have been an abrupt prominence at the place of fracture, and the spiculæ might have been felt through the soft parts. To have suspected the curvature to have been an original deformity, there should have been no motion in the part; neither was there any tumefaction in the limb which could render the case deceptive.

The symptoms then of pain and loss of power in the limb, its preternatural curve, which could be increased or diminished optionally, and the disposition in the bones when straightened to return to their flexed position, unattended by crepitus, or that circumscribed projection which a displaced fragment of bone would produce, were my reasons for considering this injury as a *bending* of the bones.

This case happened five years since. Not recollecting to have met with any particular description of such an injury, the works of several authors on fractures, &c. were consulted without finding any thing more on the subject, than some mere transient remarks. Since that period other cases have occurred nearly

similar to the one just related, with the exception that in one instance the curvature was more prominent nearer the wrist, and in another the depression was anteriorly, and confined principally to the radius.

Boyer mentions in a cursory manner the occasional existence of this injury in children, without describing either the particulars of it, or the manner in which it should be treated. It has long been known that the bones of infants are much less fragile than those of adults; being more vascular and containing a greater proportion of organic matter, and less of the calcareous earth (phos. of lime,) they frequently yield to injuries which, if offered to that of an adult, would produce a fracture: But the extent to which this deformity in some cases really exists has been too much overlooked. There is such a derangement in the texture of the bone occasioned by this bending, that it is as incapable of performing its office as though it had been broken; and if proper means were not taken to retain the limb in its natural shape, permanent deformity would result.

The manner in which, the texture of a bone thus bent is deranged may be best understood by bringing it to a familiar comparison. Wood somewhat resembles the long bones in its fibrous structure; and we all know the appearances presented by a tender twig which has been bent by force. Similar to that is the appearance of a bent bone. And to that partial division which we find in an older branch to which force has been applied, may be compared also the incomplete fracture which is occasionally to be met with in children further advanced in age, and will be noticed hereafter.

It has been mentioned above, that in these cases there was deformity in the limb, and that from the flexible state of the bones, it could be obviated by straightening them with the hands, but that when they were removed, the curvature was disposed to return, or that the least untoward motion would again produce the deformity. It is certain therefore, that if the limb were left in this unprotected situation, under an idea that artificial support was unnecessary, it would soon meet with some resistance to force it to its former crooked position: it is of importance

therefore that an arm thus injured be well protected by surgical means: yet scarcely any notice is taken of the injury. Underwood, who treats so fully of the diseases of children, generally, in speaking of fractures, says, that "the bones are easily bent or even broken. The former is very readily restored." As no directions, therefore, are here given, we may infer that it is necessary only to remove the deformity with the hands, and leave the rest to nature.

The proper plan is to straighten the limb, and apply splints, &c. and dress it in other respects similar to a fracture. In the course of three weeks the bones will have recovered their strength sufficiently to admit of the removal of the splints.

The process by which nature effects the restoration of a limb thus injured is, I presume, by the deposition of new osseous matter through the deranged structure, though not in such profusion as we sometimes find surrounding the fractured extremities of a bone constituting the *callus*.

PARTIAL FRACTURE.

There appears to be another injury to which the bones of children further advanced in age are liable. It is an *incomplete* fracture of the bone; not in the sense in which authors formerly used the expression, and which Boyer discards as incorrect, but in the true and literal meaning; a *partial* or *incomplete* fracture of a single bone or of both.

The subjects of these cases are generally under the age of ten years; and the causes which produce them are such as give rise to the common kind of fracture. The usual symptoms also of a broken bone occur, with the exception of crepitus and derangement of fragments. The patient is deprived of the use of the arm; it is painful and deformed; but instead of a curvature as in the preceding cases, there is an angle formed opposite the place of fracture. The projection of bone can be increased in one direction only, and attempts to remove it by straightening the limb frequently produce a complete division of the bone into two portions, accompanied by a noise which in some instances may be distinctly

heard, after which the crepitation may immediately be felt as in any other common case of fracture.

The pain, loss of power in the limb, and *angular* deformity, with motion just at the injured part, are my reasons for considering the bone fractured; and the impossibility of producing crepitus in the first instance, and the facility with which it may be effected after the case is reduced to the state of a simple *complete* fracture, which occasionally happens in attempts to straighten the limb, are sufficient reasons for supposing that the bone was not completely separated into two fragments originally. The symptoms are, indeed, so unequivocal as to leave no doubt, I think, as to the nature of the case. Dr. Parrish informs me that he has frequently seen cases of this kind, and that in attempting to lessen the deformity by straightening the limb, he has distinctly heard the bone crack and felt it give way, and that then crepitus could be perceived as in ordinary cases of fracture.

I am aware that Boyer and others do not believe that a partial fracture of the bone can happen, and he speaks of it as "an occurrence utterly impossible."*

That a partial fracture of the bone cannot take place in an healthy adult is very possible, but that it may exist in those of a young person is also so.

Phosphate of lime is the principal earth in bones, and is generally considered as the ossifying substance. It exists in very considerable quantities in the bones of old people, which is said to be the cause of their fragility; there is less of it in the middle aged; and a comparatively small portion of it in those of infants. The bones of the fœtus are said to contain about one third part of earthy matter, in the adult nearly one half, whilst in old age it composes the greater part.† To this is attributed the fragility of the bones of old people, and to their loss of vascularity the comparative tardiness of re-union.

If then it be acknowledged that the disposition in the bones of infants to bend, rather than break, arises from a deficiency of the calcareous earth in their structure; and that the fragile state of

* Hartshorne's edition of Boyer on the Bones.

† Johnson's Animal Chemistry.

those in old people is owing to a superabundance of it; it is very rational to suppose that when they contain rather more than infants', and less than those of adults, there may be a partial separation by fracture.

I have frequently procured the extremities of young subjects, and applied sufficient force to them to distort the limb very considerably; and have then denuded the bones and discovered that they were either simply bent or partially divided. These experiments have not been made on subjects who had laboured under diseases, such as might have predisposed them to any particular softness of the bony structure. I however recently repeated this experiment upon the extremities of an infant six weeks old, whose mother had been labouring under the lues ven. during the latter months of gestation, and since her parturition. In this case the bones were as brittle as those of an adult.

It is sufficient, however, to say, that so far as the symptoms above described are indicative of it, such an accident does occasionally take place.

The part most liable to this kind of fracture is, as in the former cases, the fore-arm. I do not recollect to have seen a case, where either this or the first mentioned injury was suspected to have taken place in any other bones except the radius and ulna. There appears to be no reason why it *could* not happen also to the os humeri,* or long bones of the lower extremities; but if it did, in all probability the weight of the limb would completely separate the fragments before an examination could be made.

The mode of treatment in these cases is such as is adopted in common fractures; but it would be advisable to avoid if possible separating the bones into two fragments; since in the former situation, there would be necessity for little more than lateral pressure, which if gradually increased would in time remove all deformity. Notwithstanding, however, the greatest caution, the division is apt to become complete, and additional deformity ensue from the bones losing what little support was left.

* See plate, fig. 4, in which this partial fracture was effected in experiment with an os humeri.

FRACTURE OF THE THIGH BONE IN CHILDREN.

Though the two preceding species of injury are peculiar in children under the age of ten or twelve years, yet complete fractures are by far the most common accident. When the force is gradual and applied to no particular part, but to the limb generally, either of the two former may occur; but when the violence is great, and offered directly to one point and unequally counteracted, a complete division of the bone will be most likely to ensue. A child falling upon the palm of the hand may receive either of the two former injuries, but a violent blow from a stick, or some other direct force will probably always produce the latter. It is hardly to be expected, however, that any other than complete fracture will be met with in the thigh bone, or tibia and fibula, for reasons already assigned.

It is customary for most of the accidents which occur in the neighbourhood of the Pennsylvania Hospital to be taken there immediately; more particularly by the poorer classes. Among the variety, fractures of the thigh bone in very young children were not uncommon. From the opportunity I have had of treating such accidents whilst connected with that institution, I am induced to offer a few remarks on this subject; particularly as there appear to be no directions given in the systematic works for the variation in the treatment of such cases from the ordinary fracture of the femur in the adult.

Many circumstances combine to render the treatment of such cases extremely difficult, if not impracticable by the usual method of extension and counter-extension. Before entering upon this subject, however, a conviction of the decided advantages which the plan recommended by Desault, has over every other, induces the acknowledgment, that it may be made to fulfil every object for which it was intended; and with the improvements superadded to it in this country, may be justly considered as a *perfect* apparatus. It is true there are occasionally cases to which it cannot conveniently be adapted, but they are very rare; and when they do occur, may be managed by some other temporary contrivance. Much time and thought have been occupied in attempts to invent new splints, &c. for the treatment of fractures of the

thigh bone, whilst half of that labour would have perfected them in the proper and succeseful application of those long since known. That a limb may be made as long, as straight, and as useful as originally after a fracture of the femur, is daily proved by the practice of the Pennsylvania Hospital, where the treatment by extension and counter-extension is employed to the exclusion of every other; and without exciting in the mind any of those horrid imaginations which some would feign induce us to believe were the natural consequences of such "discipline." It is the opinion of very many even at this day, that the present plans of treatment are extremely defective. That eminent surgeon Mr. J. Bell, asserts "that the machine is not yet invented by which a fracture can be perfectly secured." Another has the candour to acknowledge, after experience in "not less than a thousand fractured limbs," that notwithstanding every possible attention to fractured legs, it not unfrequently happened that, in the end, the tibia is bent more or less forward at the broken part.* These cases were treated by the ordinary method of placing the limb on the side, in the flexed position, according to Mr. Pott's method. A third surgeon says, he is disposed to believe, that most surgeons consider those cases singularly fortunate which terminate without lameness or deformity.† It is reasonable then to infer that their want of success arises from their general belief in the impracticability of confining a patient on his back, and maintaining a permanent extension upon the limb, until the bone shall have united.

Successful, however, as the usual means of effecting this are, in general, there are many objections to their use in fractures of the thigh bone in children. Luckily this accident rarely happens, though it does so sufficiently often to deserve some attention.

The absolute importance of *rest* in such cases may be impressed upon the mind of one capable of reflection, but it cannot upon that of the tender infant. Extension from the ankle and counter-extension against the tuberosity of the ischium, may be easily effected where the skin can be kept dry and free from

* Med. and Phys. Journal, vol. 32, (1814.)

† Med. and Phys. Journal, vol. 15.

excoriating matter; but in the child who has not yet acquired sufficient command over its own system to prevent involuntary evacuations, particularly from the bladder, it is impossible to employ it without excoriation. The receptacle which a counter-extending band, passing over the nates, would form for feculent matter, is another great objection to its use. If then a plan of treatment can be pursued in which the necessity of maintaining extension from these two points, is obviated, and in which a little motion from the restlessness of the child will not occasion a displacement of the fragments, the principal difficulties will be overcome.

The symptoms which call for counteracting force in adults, are the great contraction of the numerous and powerful muscles which surround the thigh bone, and which constantly tend to keep the limb permanently shortened; but in children very little exertion is necessary to command this defect, and more attention is requisite to avoid lateral deformity.

The plan of treatment to be here suggested, is little more than a different application of the common principles of extension and counter-extension. The method of applying these by Dessault's splints is necessary where the muscles are large and strong, but in an infant, or in a child of three or four years of age, they can exert very little force; and may be managed without the use of the extending bands. Sufficient extension may be kept up by lateral pressure to avoid the least deformity in the limb. The means to effect this are not by a few strips of shingle bound around the thigh alone, since they would not prevent the weight of the foot from turning out the toes and producing permanent lameness; but by a *long* splint. A piece of shingle, with sheep-skin glued on one side, and slit into joints, or double pasteboard, cut to extend from the great trochanter to about an inch above the ankle joint, and of sufficient width to inclose about one half of the thigh, and gradually tapering to the lower extremity, somewhat in the shape of the limb: a second is made to extend anteriorly the whole length of the thigh, and so wide as, with its fellow, nearly to incase it. A roller being passed from the toes to the hips, the limb is to be placed on the long splint, with tow intervening. In this

situation the leg is to be extended and kept so by an assistant, until the bandage is continued over the splint. The anterior one is then to be applied, and bound also upon the thigh. The most convenient mode of dressing such young patients, is by placing them on a table, and bringing the limb over the edge. In this position the *roller* may be applied with greater facility than the many-tailed bandage. The patient may then be placed on a mattress, or on a few blankets folded together. At the expiration of about four weeks, the dressings may be removed; and if proper attention has been paid to the case, the bone will be found to have united without any deformity; unless there has been some peculiarity in the patient.

The method just described was employed in the case of two or three infants where the fracture was not very high up; but it is not so applicable when the bone is broken near the neck, or in children further advanced in age. In such instances two splints, shaped similar to those of Dessault, and applied in the same manner, omitting the extending and counter-extending bands, are the most convenient apparatus. The bandage to confine the limb to the pelvis is here indispensably necessary. During the first week or ten days the dressings should be disturbed as seldom as possible, after which period, the fragments being blunted by a deposition of new bony matter, the limb may be dressed as often as the case requires without giving irritation to the surrounding parts. Clean bandages, &c. should be applied much more frequently than to adults; perhaps once in two days is not too often.

A fracture of the thigh bone *in the adult* frequently proves a great shock to the system; considerable fever occasionally supervenes, the stomach becomes irritable, and the bowels disordered. From these unpleasant symptoms children are comparatively exempt; so that their sufferings are scarcely more than local.

EXPLANATION OF THE PLATE.

Fig. 1.—Is intended to represent the arm of a boy near four years of age, who had fallen upon it from the fourth rail of a fence. The force was not so violent as to produce a fracture of the bones, but sufficiently so to derange the texture of them, and to occasion the deformity as delineated in the plate.

This case happened in the child of a labouring man by the name of Hanse, (residing at the corner of Vine and Juniper streets), whilst writing this paper, and afforded me an opportunity of exhibiting it to one of my colleagues, Mr. B. H. Coates,* and to Mr. De Graffenreidt, students of medicine; and in the presence of them and others of placing a sheet of paper against the arm, and *tracing* accurately an outline of it with a pencil, before the deformity was removed. From this again was carefully taken the outline in the present plate.

The several cases of this nature might have been drawn out individually, and inserted at length; but it was not deemed necessary, since it would be a mere repetition of the annexed described symptoms.

Fig. 2.—Is not intended to represent any particular case, but is introduced merely to illustrate, by comparison with figure 1, the different kind of deformity; the first occurring where the bones of the arm have been bent; and the second where they are supposed to be partially divided by fracture.

Fig. 3.—Drawn from the bones of the fore-arm now in my possession, where the experiment was tried by applying considerable force to the limb after death. The radius is partially divided by fracture, and the ulna simply bent.

Fig. 4.—An os humeri subjected to the same experiment, also in my possession.

Art. II. *On Wounded Arteries.*—By J. B. Davidge, A.M. M.D. Professor of Anatomy in the University of Maryland.

This is the third time that I have called on the attention of the profession, in respect to wounded arteries; my apology is in the importance of the subject. I wrote two letters, published in my Physical Sketches, by way of criticism, on Dr. Jones of London, and afterwards a more formal analysis, published in the same volume, on a supposititious case of a wounded artery given to the public by Dr. Dorsey of Philadelphia.

In his Elements of Surgery, Dr. Dorsey renewed the subject under discussion, by repeating his own imaginary case, and by a reference to the experience of Petit and Scarpa.

By this reiteration of his own case and his quotation of Petit and Scarpa, the reader may be led to the belief that there is something of fact in his assertion. For he could scarcely conceive that any man of professional reputation would gravely and

* Now Dr. Coates.

seriously assert a thing to be true or untrue of which he had no personal or immediate knowledge; and of which he *could* have no knowledge. Yet such is the fact.

As the reader probably has seen my review of Mease's case, published in my Sketches, I here barely make reference.

Not contented with what he had, without the slightest positive or even probable evidence, asserted in the case of Dr. Mease, Dr. Dorsey, in a stronger and bolder tone, calls the attention of the reader to Petit and Scarpa in confirmation of what he had advanced. To readers, in any degree acquainted with the phenomena and nature of wounds of arteries, any thing more than has been already advanced may appear superfluous. But, perhaps, even to such, the boldness by which the author of the Elements appealed to the authority of the French physician and Italian professor may make it proper that I address a few words.

Whether the canal of a wounded artery be obliterated, above and below the point wounded, is, it is obvious to every man of the most common and ordinary understanding, *to be ascertained only by dissection after death.* All speculation on the subject, during life, is frivolous and below serious consideration. The *injection-pipe* and the *dissecting knife* can *alone* decide the question. In this, I believe, I shall not be contradicted by any man in any degree conversant with the subject.

It is said of Pliny, whenever he was about to narrate an affair of which there was any doubt entertained, of which he had no personal knowledge, that he always guarantied himself by the remarkable sentence, sic dicitur—so they say. But the author of the Elements feels himself under no such restriction. He unreservedly avers that the thing is as he chooses it should be.

"But in some instances the artery heals, as in the experiments upon brutes related by Dr. Jones, and then the cicatrization is *completed without a destruction of the arterial canal.* Mr. Petit records an *instance* of this, and in Professor Sarpa's learned treatise on aneurism, I find another very *decided instance* in which this mode of union was effected in the brachial artery of a man. Scarpa *denies* that when the artery heals in this manner the cure is *radical.*" Dorsey, vol. i. p. 40.

As of the case of Petit, referred to above as an instance of a wound in an artery healing without a destruction of the arterial canal, neither the author of the Elements nor myself could have any personal or certain knowledge, I will, for the satisfaction of the reader, repeat the decision of a very competent tribunal, the decision of the Academy of Paris. This tribunal, it may be presumed, before giving its decision, was careful to inquire well and seriously into the nature and circumstances of the case. It was perfectly aware that the point was a disputed one, and that a very dispassionate examination would be expected from so grave and learned a body. The determination of the Academy of Paris, is as follows:

" Practical surgery must acknowledge, as a fact undeniable, that no *radical* or *complete* cure of an aneurism ever takes place without an *obliteration* of the canal of the artery, or a conversion of it into a *solid* and *ligamentous substance* both above and below the point where the wound is inflicted, or where the injury is. It is a great error to think that in compressing the dilated coats of an artery they are strengthened, or that they are so fortified as to be adequate to resist the impulse of the blood as before the dilatation. In wholly denying this Petit and Foubert were not justified in admitting the formation of a coagulum which could close up the aperture of the artery, and render it competent to resist the force of the blood. The numerous experiments made by Haller on the mesenteric arteries of frogs amount to nothing, as from them no deduction can be made favourable to the formation of this pretended clot."*

* *Traitement Général des Anévrismes.*

" La chirurgie pratique doit admettre un fait incontestable que la cure complette et radicale d'un anéurisme n'a jamais lieu, n'emporte oú, sans oblitération, sans dégénerescence ligamenteuse et solide de l'artére au-dessus et au-dessous de sa solution de continuité. C'est une grande erreur de penser qu'en comprimant les tunique dilatées d'une artére on les fortifie, ou qu'on les rende propres à resister à l'impulsion du sang, comme avant leur dilatation. Tout en niant ce résultat Petit et Foubert n'etoient point fondé à admettre la formation d'un caillot qui bouchoit l'ouverture de l'artére, et la rendoit propre à resister a la forcè du sang. En vain Haller fait beaucoup d'expériences sur les arteres mésentérique des grenouilles, on n'en peut rien conclure sur la formation de ce prétendu caillot." Journal Général De Médicine. tom. 23. p. 220.

A body of men more competent and professionally prepared to examine into the particulars of the case, in all probability, is not furnished by any medical school of the world. This academy decides that Petit and Foubert are not justified in forming the conclusion they did. They do not say upon what grounds, but there can be no doubt that the decision was made on the circumstances of the case. Every case must be determined by the nature of its own peculiar phenomena. Yet a professional gentleman in Philadelphia publishes to the world, that "Petit records an instance of the wound in an artery healing *without* an *obliteration* of the arterial canal." Not that Petit had recorded a case which he *believed* to be without an obliteration. In this there would not have been force enough. The reader might conclude that Petit was under a wrong impression; therefore the author of the Elements assumes the responsibility to himself, and announces to the world, the fact is so! "Petit records an instance!!"

"And in Professor Scarpa's learned treatise on aneurism I find another very *decided instance* in which this mode of union was effected in the brachial artery of a man; Scarpa denies that where the artery heals in this manner the cure is radical."

This is really a novel manner of establishing a thing on authority, to inform us that the very authority quoted, denies the thing asserted. Dr. Dorsey asserts, that Professor Scarpa has in his learned treatise a *decided instance* of a wound in an artery healing *without* the *obliteration* of the canal, and lets us know at the same moment that Scarpa denies the possibility of a *radical cure* taking place under any such circumstances. Is there any cure that is not radical?

To oblige the reader I will give him Scarpa's words from Dr. Dorsey's Elements, and then I will furnish him with the case of the wound in the brachial artery; demonstrate to him that the man died without any cure at all, and finally put him in possession of the decision of the Academy of Paris, that the case is not to be put on the list of radical cures.

At page 177, vol. ii. the author of the Elements tells us, that Scarpa asserts, as "a *certain* and *incontrovertible* fact, in practical surgery, that a complete and truly radical cure of an

aneurism *cannot* be obtained, in whatever part of the body the tumour is situated, unless the ulcerated, lacerated, or wounded artery from which the aneurism is derived, is by the assistance of nature, or of nature combined with art, *obliterated*, and converted into a perfectly *solid*, *ligamentous substance*, for a certain space above and below the ulceration, laceration, or wound."

Can any form of words be more expressive, or less equivocal? However, with these words, as strong and unreserved as they are, the author of the Elements was acquainted. They are taken from his own volume. But, notwithstanding this strong and expressive style of Scarpa, Dr. Dorsey asserts that in Scarpa's learned treatise a very *decided instance* of this union, without obliteration, is to be found!!

"Scarpa, nevertheless, asserts that this circumstance of the formation of a clot, is, at times, to be met with, and that he has seen a case where a clot of blood had not only closed the opening in the brachial artery made by a lancet, but that its *apex* or *summit* continued to be interposed between the lips of the wound, and that it was converted into a hard substance, so strongly adhering to the exterior of the vessel that in *viewing from within* a solid cicatrix could be seen, or at least something resembling it."*

Here the reader will perceive that Scarpa merely asserts that he had seen a coagulum interposed by its summit between the lips of a wound in the brachial artery, and that it continued to be interposed. How long? Surely, if the patient recovered the clot of blood, as a clot, could not remain adhering to the exterior of the vessel with its apex interposed between the lips. It would in part have been melted down, and in part been removed by the absorbents; at any rate, have lost its character of a coagulum in that of organization.

* "Neanmoins Scarpa dit que cette circonstance se recontre quelquefois Il a vu un cas où un caillot de sang avoit non seulement formé l'ouverture a l'artére brachiale blessée par une lancette, mais encore s'ètoit interposé par son sommet éntre les lèvrès de la plaie, et s'étoit converti en une substance dure, si fort adhérente à l'exterieur de ce vaisseau, qu'en regardant celui-ci en dedans, ou voyoit une cicatrice solide ou quelque chose qui lui ressembloit." Journal Général De Médicine, tom. 23. page 220.

And how could professor Scarpa have ascertained how the thing appeared upon *looking within* the artery? how know that it had the appearance of a cicatrix? The artery of a well or living man certainly was not laid open that curiosity might indulge itself.

The fact is, the patient died, and upon dissection the coagulum remained with its summit interposed between the lips of the wound. And upon splitting up the artery and *viewing* the aperture the clot was seen adhering and presenting an inner circumscribed surface resembling a cicatrix.

The *inner surface* could not be seen except the artery were laid open, and except the inner surface were seen how could Scarpa say, "upon *viewing from within*, a solid cicatrix could be seen." It cannot be presumed that Scarpa laid open the artery of a living and recovered arm. What could have been his object?

It is not surprising that the judicious writer in the Institutes should say, "we are not premitted to place it (this case of Scarpa) in the order of radical cures." Surely not; seeing that the patient died within a few days after the infliction of the injury.

But this is a case, a most "*decided instance*, of a wounded artery healing without an obliteration of its canal!!"

This criticism is for the benefit of the living, not to reflect on the dead. I do not take up my pen to commend or to censure those who are no more. I write to improve science; at least to animadvert on folly, and to suggest to writers that to misrepresent authorities is not to give ourselves the best position before the public, either as to morals or intelligence; while some may copy, others read. Let it be recollected that the day of gratuitous assertion is with those beyond the flood.

The language of science should, at all times, be plain and intelligible. Its appeal is to facts; its object the discovery of truth.

Art. III. *Observations on Cataract, and the various Modes of operating for its Cure.* By G. Frick, M. D. of Baltimore.

(Continued from page 491, Vol. III.)

From what has been said respecting the great variety of Cataract and its manifold complications, may be explained the various success resulting from the different modes of operating for its cure. Whether the disease be capable of removal by the simple power of medicine, is a question we cannot take upon us to decide: until we shall have attained to some more intimate knowledge of its essence, and the action of its causes, we can expect but little improvement in its mode of treatment.

There can be no doubt, that cataract has been often times checked in its progress by the administration of remedies, adapted to correct that peculiar diathesis of the system, from which it has originated: but there is no single instance on record, where the disease can be said to have been cured, or, in other words, where the opaque lens has again become transparent, by such means.* It was at one time very much the fashion to resort to

* It has sometimes happened that persons affected with cataract, have suddenly regained their vision, or the pupil has taken on its usual transparency, and the opacity has entirely vanished. This case can only be explained by the spontaneous depression of the lens. Boyer relates a curious instance of this kind. "An aged gentleman, an advocate, laboured under cataract in both his eyes, one of which was operated upon unsuccessfully. The other was thought by the most celebrated surgeons, to be a case not warranting any operation. He was blind 25 years. One day when accompanied by his guide he was walking through the street, he was astonished to distinguish all at once, the different objects which surrounded him. Surprised at a thing as happy as it was unexpected, he came to the professor and asked him to examine his eyes. The crystalline was found detached in $\frac{4}{5}$ths of its superior margin, and appeared suspended merely from the inferior. It was slightly agitated on moving the head violently. The superior half of the pupil was clear and transparent, the inferior still obscured by the opaque lens." *V. Maladies Chirurgicales*, vol. 5th, p 509.

Spurious cases of cataract (where the disease is seated without the lens) no doubt often yield to the power of art. Thus, I have seen a patient under iritis, who had the pupil so completely filled up with coagulable lymph that his vision was entirely destroyed. By the aid of a little mercury, this was removed in a few days, and the patient enjoyed his vision as before.

certain external and internal medicines for the cure; and belladonna, pulsatilla nigricans, æther, mercury, &c. have each in their turn, received the highest encomiums. At present they are entirely laid aside; and the operation is the only means, by which we calculate upon restoring again the organ to its healthy functions. Even this, not withstanding the degree of perfection it has attained in modern times, is not in all cases practicable; in the most favourable too, so many untoward and unforeseen circumstances often occur, as to mar completely the success of the best contrived operation. This failure, no doubt, is often to be ascribed rather to the improper choice, than to any imperfection in the operation itself: and the result can never be otherwise, as long as surgeons continue to operate after one method only, to the neglect of all others. Thus, it is very common to hear one surgeon ask another after what manner he operates for cataract; or, if we look into a treatise upon the disease, we shall find the author constantly advocating one method, to the exclusion of the rest. It is very evident, however, that no one mode can ever be adopted indiscriminately in all cases, but that our operation should always be suited to the particular circumstances of the disease. The success does not so much depend upon the extraordinary dexterity and ability with which the surgeon is enabled to perform any single operation, as upon a judicious selection of that which is best adapted to the particular case before him, and a moderate degree of adroitness in all the several modes of operating. It is this, as Dr. Beer very justly observes, which distinguishes the mere quack from the man of science.

Before describing these various methods, it will be necessary to notice briefly a few circumstances to be attended to previous to the operation. It was the custom formerly, before submitting the patient to the operation for cataract, not only to subject him to the most rigid diet, but to order repeated purgatives, bleedings, &c. This practice being now found prejudicial, is entirely laid aside: for by weakening the constitution, it only rendered the patient more irritable, and of course the more susceptible to the very inflammation it was intended to correct.

When the time is fixed upon for the operation, this should

never be protracted to a more remote period; as this only increases the anxiety and distress of the patient. It is needless to remind our readers of the influence which the mind exerts upon the success of our operations.

Great care should be taken that the patient does not overload his stomach with food previous to the operation. He is frequently guilty of this excess, to prepare him for the starving plan which is to follow. Where costiveness is present, a slight laxative may be administered.

In all ancient treatises on cataract, the question is asked, what is the most proper season of the year for operating: and the answer is universally, in the spring. Now, this is exactly the worst time; as from the frequent vicissitudes of weather, the patient is much more liable to catarrhal and rheumatic affections. It is for this reason, that we give the preference to the driest and warmest or coldest season.

The operation should never be undertaken during the prevalence of any epidemic.

There are three modes of operating for the cataract, now in use. Extraction, *couching* or *depression*, and the *breaking up of the lens*, or what is termed by the German writers *Keratonyxis*. The ancients, who had but very crude notions respecting the nature of the disease, and who supposed it nothing more than an adventitious membrane filling up the pupil, shaped their instruments and directed their efforts in such a manner as would best enable them to tear away or destroy this film. Hence, they employed either round needles, about which they supposed they could wind this pretended membrane, or they made use of some sharp instrument, in order to cut away the small filaments, by which, they believed it was united to the ciliary processes. Another invented a species of forceps, with which he might seize this membrane, and bring it altogether without the eye.* Daviel is to be considered the first, who operated through the cornea with a distinct view of extracting the cataract. Although the operation was for a long time considered possible, and practised by a great

* Various instruments of this kind may be seen depicted in Heister's Surgery, or the Memoires de l'Academie de Chirurgie.

number of surgeons upon the dead body, no one could be found bold enough, to attempt it upon the living, until Daviel, in 1745, undertook the extraction, and with perfect success.* Petit, St. Ives, and others had performed the same thing long prior to this time; and there are not wanting some who have awarded the claim of the discovery to these gentlemen.

Thus, Petit, being called to a priest who had suffered a severe contusion of the eye, discovered upon opening it, that the lens projected into the anterior chamber, and was so large, as to press backwards the iris, and obscure completely the vision. From the excessive pain and inflammation it excited, he resolved on extracting it; viewing it in its present situation as a foreign body, and the cause of all the mischief. He had little idea, however, that by this operation, he would restore his patient again to sight. With a common lancet, he made a puncture into the cornea, and took away the lens; but how great was his astonishment to find in a little time, that the pain had subsided, and the person to enjoy his vision as complete as before the accident.

The multiplicity of instruments was the greatest objection made to Daviel's method: but this was soon remedied by the simplicity which La Faye† gave to the operation. It would employ too much of our time to dwell upon the successive improvements made upon these instruments since his time, by various French, German, and English oculists; those of Richter and Wenzel alone maintain a place in modern practice. It is well enough observed by professor Himley, that a virtuoso in the art may operate with any instrument: it is not to be denied, however, that for those who are not really virtuosi, one form of instrument is very preferable to another. We shall sum up in as concise a manner as possible, the most important properties of a good knife. 1. It should be so formed, that the incision of the cornea may be completed with a single stroke of the instrument. Its greatest breadth should therefore equal at least half the diameter of the cornea, not less than three lines.

2. The greatest breadth of the knife should not be too far from

* Memoires de l'Academie de Chirurgie, t. v. p. 369.

† Memoires de l'Academie, t. vi. 304.

the point, lest this wound the nose before the cornea is quite divided.

3. The knife should be made to widen gradually and equally from the point, not too rapidly, however, as this would cause it to penetrate with too much difficulty through the cornea.

4. The point of the instrument must be made very sharp, and at the same time sufficiently strong; the lancet-shaped is the best form.

5. The back of the instrument should be straight and not curved; for when of the latter description, it is impossible to make the opposite wound through the cornea, sufficiently high, and your incision will always turn out too small.

6. Both sides of the knife should be gently convex; as well to increase its strength, as to fill up the wound, and thus prevent the escape of the vitreous humour.

7. The cutting edge of the knife may be either straight or curved. Barth, Schmidt, and Beer's knife is straight, Himley's curved.*

It is of the greatest importance that the blade of the knife be always permanently affixed to the handle. Professor Barth was always in the habit of carrying his blades wrapped in a piece of paper in his pocket, and two or three handles in his instrument case. It happened to him once, in operating for a cataract, that the blade slipped from the handle, and he was obliged to finish his incision by grasping hold of the former, and drawing it firmly through the cornea. Now, such an accident, though of trifling moment to a surgeon of Barth's eminence,

* Vid. Opthalmalogische Bibliothek. B. 3. St. 2. Wenzel's knife seems hardly to have one of the qualities requisite for a good instrument. At the same time that he made the section of the cornea, he was accustomed to dip his knife into the centre of the pupil as he passed, in order to divide the capsule; and hence it was necessary to make use of a long aud narrow knife. The Baron is said to have operated very rapidly, and to have possessed a peculiar command of himself during this operation. It is related of him, that whilst in the act of dividing the cornea upon a nobleman the patient arose from the chair; but the Baron, not at all disconcerted, arose also, followed him around the room, and finished the section without withdrawing his instrument. The triangular knife of Beer is generally employed in the London Eye Infirmary.

might prove highly dangerous, should it occur to one young and inexperienced in his profession.

In operating for cataract, the patient should be seated in a chair without a back, in such a manner as that his head be on a level with the breast of the surgeon. In this position, the operator sits with the greatest ease, and is enabled to see without any unnatural motion of his body backwards or forwards, every step of his operation. All apparatus of stools intended as any mechanical assistance to the surgeon, should be rejected. Arnemann, a German oculist, had a certain portable chair, which he carried about to all persons upon whom he intended operating. It is utterly impossible to operate in a standing position, as the surgeon, if small, must lift his arms very fatiguingly to reach the eye of his patient; or, if the reverse, must bend his body in a not less uncomfortable manner. The patient also, is very liable to faint under this operation, and should this occur, he falls to the ground, without the surgeon being in the least able to assist him. Barth chose to operate in this position, without an assistant, by fixing the head of his patient against a wall, which was lighted obliquely by a window, opening the lids with one hand, and operating with the other. This scheme may no doubt answer very well, where both eyes are perfectly blind. In most cases, however, it is impracticable from the difficulty of managing the eye-lids; and particularly where the eyes are seated deep within their sockets. Beer once operated after this manner, and with the most perfect success. Stepping in accidentally to see a person, upon whom he was about to operate the succeeding morning, he found him in the greatest anxiety and alarm, and under pretence of showing him how little pain he would suffer with the operation, fixed him in the manner above mentioned against the wall, and operated without a single person being present, and quite unknown to the patient himself. When he directed him afterwards to open his eyes, he was so struck at seeing again the different objects around him, that he had nearly fallen into convulsions.

Richter directed that the light should be made to fall *obliquely across the nose:* but, if we were to follow him in this rule, we should be under the necessity, where both eyes are to be operated

upon, of turning our patient around, at each step of the operation; and this would create great inconvenience to the surgeon, as well as unnecessary fear and apprehension in the mind of our patient. It is always preferable after making the incision of the cornea on one eye, to practice the same upon the other, and not to complete at once the operation upon the first. Richter is right in directing that the light should fall *obliquely*, but whether across the nose or temples is a matter of very little importance.

A second rule which Richter lays down in the operation for cataract is of greater weight; viz. that where both eyes are to be operated upon, *the incision of the cornea should never be practised with one and the same knife*, but that a new instrument be taken for the second incision. The reason of this, is, that the knife acquires from the aqueous humour, a dulness, which renders it unfit for immediate use: by drying it with a fine towel, this, however, wears off after a few hours, and the instrument is as good as ever.

The upper eye-lid should be elevated by an assistant, with his fore and middle finger, who supports at the same time the head of the patient upon his breast. The surgeon is in the same manner to depress the lower lid, permitting the fingers to project a little towards the ball of the eye. In this manner, he will be enabled to check the motion of the eye inwards, as it flies from the point of the knife. All mechanical means of separating the lids, or fixing the eye, should be rejected. I have heard of an oculist of Dresden, who was in the habit of raising the upper lid with a species of hook which he gave to his assistant, and depressing the lower in a similar way, excepting that to this, he appended a key or other weight. By this kind of violence, he not only created unnecessary fear and apprehension in the patient, but in most instances, the organ was completely destroyed by this very unnatural stimulus.

The patient then, being properly adjusted for the operation, and the surgeon having determined upon the choice of his knife, he is to commence his first incision, by directing the point of the instrument obliquely towards the cornea, as if to penetrate the iris. Upon this first step of the operation, hinges in a great de-

gree its whole success. Commonly, the knife is directed in a parallel line with the iris; and hence it happens, especially in elderly persons, (and these are for the most part the subjects of our operation) where the cornea is much thickened, and the anterior chamber of the eye much diminished by the absorption of the aqueous humour, that the instrument is carried for some distance between the laminæ of the cornea, and the section, on this account, turns out too small.* Too much pressure and violence must afterwards be employed upon the iris to force the lens through the pupil, and this is often the cause of perfect closure of this membrane. The surgeon is easily satisfied that he has carried the knife through the cornea, by the shining appearance of its point as soon as it has penetrated; the part buried in the cornea being always more obscured. When it has fairly pierced this tunic, it is to be carried carefully and steadily forward, in a parallel direction to the iris, and brought out in the same manner through the cornea on the opposite side. As the eye always flies from the point of the knife, in a direction inwards and upwards, it is necessary, in order that the section be made sufficiently large, that the instrument be brought to divide the cornea about one-fourth of a line *above* its horizontal diameter. It is never requisite that the incision comprehend more than one half the cornea, though no harm can result from its accidentally being made greater. Care must be taken in this part of the operation, that the edge of the knife be not suffered to vacillate between the iris and cornea, else, the former becomes entangled with the instrument, or the aqueous humour escapes.

As soon as the point of the knife has penetrated the opposite side, the surgeon should rest a moment in the operation, until that spasmodic action of the eye, which has originated from the irritation, has subsided. This action is at times so powerful, that the instrument is almost dragged from the hands of the operator, unless he is careful to hold it very firmly. The incision is now to

* "Mon opinion est, que sur vingt yeux perdus, après l'operation de la cataracte par extraction, dix sept verroient si l'incision placée convenablement, avoit eu une ligne de plus de longueur." &c. Demours, Op. Cit. vol. I. p. 511.

be completed by pushing the knife steadly forward. Younger surgeons, for fear of wounding the nose by this motion, use the knife as a species of saw; but this is in the highest degree injurious; as the membrana hyaloidea is always torn by this action; and when the operator afterwards attempts to propel the lens, the vitreous humour escapes in its stead. They are generally guilty too of completing this part of the operation in too great a hurry. Indeed, this was the great fault of the elder Wenzel, who never waited until the spasmodic action of the eye had abated, but carried his knife with the greatest precipitation through; so that very commonly he cut off a great part of the iris, and a large portion of the vitreous humour was sure to be protruded. This is always the most difficult step of the operation, as the surgeon must have his attention divided between the point of the knife, lest it touch the caruncula lachrymalis or root of the nose, and thus cause the patient involuntarily and convulsively to withdraw his head from the operator; or upon the edge of the knife that it do not divide the iris which has fallen before it. The first is easily avoided by depressing the handle of the instrument towards the temples; the second by rubbing gently upon the cornea with the fore-finger of the left hand. This speedy mode of operating has another disadvantage: the conjunctiva, instead of being divided, is torn away from its attachments with the sclerotica. It is more especially apt to occur in elderly persons, where this tunic is thicker, tougher, and more loosely attached, than in the younger subject.

Great care must be taken too, to render the incision smooth and even; for if it is at all ragged, the parts never fall into equal contact, and reunion can never take place per primam intentionem. The wound will always take on suppuration; the consequence of which will be an incurable cicatrix, with more or less of nebula extending around it. This may cover the whole pupil, and thus frustrate completely the operation; or it may only conceal the inferior half. Even this is a serious inconvenience to the patient; for although he may be enabled to read the finest print, when placed immediately before his eyes, he will be apt

to stumble over every step or stone in his walk, from his incapacity of seeing the ground.

Where the eye is very protuberant, it would be preferable to make the section of the cornea in its outer half, as practised by Wenzel. If the operation is made in the ordinary manner, the lips of the wound will be very liable to overlap each other, and a large cicatrix will be the inevitable consequence. It would be of much advantage in all our operations, if the section could be made in this manner, as it heals far more rapidly than in the common mode; but this is rendered quite impracticable from the projection of the eye-lids.

The cornea being properly divided, and the eye allowed a few moments repose, our next object is the *opening of the capsule*, to give egress to the lens. Wenzel, as we have already noticed, was in the habit of doing this, whilst he made the section of the cornea, by inclining his knife gently backward, as soon as it had arrived opposite the pupil; but this mode should never be imitated. Others have suggested different methods of extracting the lens together with its capsule: and though experiments of this kind very easily succeed on the eyes of several brute animals, as of the hog, ox, &c. we are not equally certain that the practice can be extended to man. Richter was the first to advise this practice in 1775; but it fell again into disrepute until revived in 1799 by the celebrated professor Beer.* He very justly supposed, that by extracting the capsule, one of the chief and most frequent causes of secondary cataract would be prevented. He was, no doubt, frequently deceived in supposing he had accomplished this object, when on putting the lens in a little water, he observed something like the capsule, but which in fact was nothing more than the superficial layers of the lens, separated by the fluid, which had penetrated its substance. It may be questioned too, whether many cases of secondary cataract, which are attributed to opacity of the capsule, are not owing rather to the effusion of coagulable lymph filling up the fossula left by the lens.†

* Methode den grauen Staar sammt der Kapsel auzuziehen, &c. Wien 1799

† Schmidt über Nachstaar u. Iritis nach Staar operationen. Wien 1801.

The Cystotome caché of Lafaye, Barth and others for opening the capsule is to be rejected in toto. This is like operating in the dark; as the surgeon is unable to distinguish what he is about. Proceeding to the third step of his operation, the withdrawing of the lens, he finds perhaps, after doing much violence to the iris by his pressure, that the capsule is not at all divided, and he is obliged to repeat his operation.

This may happen, if the capsule be in the least degree firmer than natural; for having no point d'appui from behind, it flies before the cystotome, and remains uninjured. The most simple and safe mode of proceeding, is to introduce an arched or straight needle, somewhat smaller than that which is commonly used for couching, through the section of the cornea, taking care to lift this as little as possible, dipping it down through the centre of the pupil, and making a crucial incision of the capsule. Professor Beer advises three or four parallel incisions to be made through the anterior capsule; and these crossed again by as many more, so as to cut out a number of small parallelograms (rautenæhnliche Laeppchen). These he thinks will necessarily escape afterwards with the lens, and thus diminish very much the chance of secondary cataract. All pressure upon the eye should be removed, and care taken in opening the capsule, that we do not dislocate the lens; for if this body do not completely fill up the pupil, the vitreous humour will protrude before it, and increase very much the difficulty of its extraction.

The pupil may so contract after the first incision through the cornea, that the opaque lens is no longer visible, and we are unable to proceed to the division of the capsule. This is commonly the effect of too great sensibility of the eye; and to complete his operation, the surgeon is obliged to turn the head of his patient from the light, or to darken the chamber, when the pupil immediately dilates. In this position, he is to divide the capsule, for as soon as the organ is exposed again to the light, the iris contracts.

If the capsule be sufficiently divided, the lens will generally protrude soon after through the pupil, by the sole action of the eye. If, upon waiting a few moments, the surgeon should find

that this reaction does not take place, or is too feeble to expel the lens, he is to assist its expulsion by pressing gently with the arched curette, the globe of the eye, over the upper eye-lid. Should the lens still show no disposition to come forward, but rather ascend in the pupil, we may be assured there is adhesion between the body and the posterior capsule, and the latter, with the membrana hyaloidea. This is an occurrence that can never be foreseen, and arises commonly, where the cataract has been the result of previous inflammation. It is by no means necessary in this case, that an union exist between the lens and anterior capsule; this is most commonly found wanting. Under these circumstances, you press upon the lower part of the eye, until the under portion of the lens becomes observable, and in that moment proceed with the curette, through the dark lunula it forms with the iris, up between the capsule so as to free the lens, and bring it forward.

It frequently happens, especially where the cataract is of the species we have denominated scabrous, that several opaque fragments of the lens are left behind, or we perceive these shelling off, before the lens has completely protruded. Most authors generally direct they should be left for absorption; it is the safest practice, however, to remove them at once with the grooved director. In many instances, it is not even necessary to introduce an instrument, as these shreds of lens will easily be expelled by simply closing the eye, and rubbing gently with the finger upon the upper eye-lid.

Particular care should be taken before the dressings are applied, that no fragments of the lens, or no fold of the iris be insinuated between the lips of the cornea that may prevent their union.

Mr. Ware has so fully treated of the causes preventing the success of extraction, that we have little to add on this subject.

We have already noticed one cause, why the section of the cornea may turn out too small; viz. where the point of the knife is carried some distance between the lamellæ of this tunic. This circumstance may be known by the greater force which is required to push forward the instrument, as well as the degree of cloudi-

ness which obscures its point. The knife should then be carefully withdrawn to the margin of the wound, and the punctuation attempted anew. Mr. Paget,* once completed the section in this manner, and without entering at all the anterior chamber; but discovering the accident, he repeated the incision through the inner lamella in the usual manner. The wound, notwithstanding, healed without the slightest opacity. We think Mr. P. would have avoided this unfortunate accident, had he attended to the rule we have already laid down, of never performing a second incision of the cornea with the same knife; as it was, no doubt owing to the dulness of the instrument, that obliged it to seek this course. It would have been more judicious too, had he permitted the separated lamellæ to unite, before completing his operation.

The point of the knife, particularly if it has pierced the cornea too near its union with the sclerotica, may become entangled with the iris: or, this membrane may fall before the edge of the knife, from the sudden escape of the vitreous humour. From the difficulty of operating under these circumstances many surgeons have advised its postponement, until the aqueous humour should have been again regenerated. It is possible, however, by carefully manœuvring the point of the instrument, gently depressing, and raising at the same time that we force it slightly forward, to avoid the iris, and accomplish the operation. The greatest difficulty will be experienced in attempting to pass it over the inner ciliary edge of this membrane. Where the iris falls before the the edge of the knife, it should be reinstated by pressing gently with the finger upon the cornea. In many cases it is necessary to keep up this pressure, until the section of the cornea be completed; and this may be done without any risk at all of injuring the iris.†

In very irritable and timid patients, it not unfrequently happens, that we have already made the punctuation of the cornea, when by a sudden and convulsive motion of the eye inwards, it

* Edinburgh Medical and Surgical Journal, Vol. IX. 1813.

† The action of the finger upon the cornea, in causing the iris to recede from the edge of the knife, seems more a mechanical than organic effect; as it equally takes place in the dead body.

flies entirely off from the point of the knife. We are then to take a new instrument, search for the old orifice and complete the section. Or, the eye may recede so far into the inner canthus, as to conceal completely the part of the cornea at which the knife is to be brought out. This accident may be prevented in a great measure by adopting Mr. Ware's method of steadying the eye by slight pressure with the finger.

Should a prolapsus of the iris occur after the operation, we may readily reduce it, by closing the eye-lid, rubbing gently but rapidly over the cornea with the thumb, and then suddenly opening the eye, so as to admit a moderate degree of light. This will cause the iris at once to contract. This accident is most liable to occur, where the surgeon has been compelled to make use of the curette for the extraction of fragments of the lens. It may happen also from a protrusion of the vitreous humour. Here no attempt should be made to bring back the parts into their natural position: the eye is to be left to itself, and the wound in general heals very kindly, leaving only a slight malformation of the pupil, which does not, however, in the least interfere with the patient's vision.*

Where the capsule is found opaque after removing the lens, we should always endeavour to extract it with a small pair of forceps. Professor Beer, in his Lectures relates the following very curious circumstance, which occurred to him in operating for cataract.

* Professor Beer has found by the result of certain experiments made upon the vitreous humour, that in no case, where it escaped was it ever regenerated; but that its place was always supplied by an increased secretion of the aqueous humour. If $\frac{1}{4}$ part of this substance only is destroyed, and the eye remains in other respects uninjured, the sight, he says, is not in the least degree impaired; but with the loss of $\frac{1}{3}$, the vision is very sensibly affected.

Richter remarks, in his Surgical Observations, that if only a small portion of the vitreous humour flows out of the eye after the operation for cataract, the patient always sees better; and this observation is sanctioned by the very extensive experience of Beer. He asks the question, how far it might prove beneficial to let out a little of this humour after extraction; and if the quantity could be duly regulated, no doubt could remain of the propriety of the practice. It is always to be apprehended, that by the convulsive action of the muscles so apt to take place after extraction, more might be forced out than is absolutely safe.

The patient was a divine, and laboured under firm cataracts in both eyes. In the right the operation was completed, as the professor expresses himself, very jovially, the patient jesting during the whole time. When he extracted the lens in the left eye, he found the pupil still obscured; and an appearance, as if a second cataract were still remaining. He was fully satisfied, that this could not proceed from any particles of the lens, or from any obscurity of the posterior capsule, as the pupil was equally dark over its whole surface; and the opacity did not present the concave form we witness in capsular cataract. He introduced a curette through the pupil, when he felt something firm like a second cataract, and pressing upon it, it became sufficiently loose to permit of extraction. The mass was flat, and resembled much in shape a common plate, which perfectly convinced the doctor that it was nothing more than the morgagnian humour, hardened into this concrete state. The pupil was not yet clear; and he was obliged to remove the posterior capsule, which had taken on the same disease. The patient was completely restored to sight after the operation.

It may happen that a slight obscurity of the capsule is overlooked by the surgeon, previous to his operation. This is more liable to occur in the cataracta scabrosa, particularly if the lens be very white. Barth makes mention of this circumstance, but supposed the capsule would so contract itself behind the iris, as to leave a sufficiently large and transparent pupil. This contraction of the capsule, however, is never to be depended upon; and it is always the safest practice to remove it. The slightest inflammation which may ensue from the operation, will unite the capsule with the uvea, and thus cause an opacity scarcely capable of removal by any subsequent efforts.*

A second and more ancient mode of operation is that of couching, or *depression of the lens.* Celsus is said to have already practised this method, and until the beginning of the last century,

* Dr. Lobstien Loebel, a Dutch oculist, has recommended and practised the extraction of the lens through the sclerotica. His mode of operating is described in the 37th volume of the London Medical and Physical Journal, which contains also some remarks on the operation by Dr. Chapman.

none other was attempted. About this time, we have seen, the French surgeons suggested and ventured upon opening the cornea for the extraction of the lens, and this continued the favourite practice of most surgeons both in England and on the continent, until the writings of Pott, Hey, Scarpa, and others brought the operation into disrepute, by the very decided preference they gave to the older, and the numerous objections they advanced against the new method.

Surgeons have in general stuck ad libitum through the sclerotica in this operation, and no rules have as yet been prescribed by any author on the subject, sufficient to direct us as to the proper manner of piercing this tunic. It is for this reason that the operation has been so often attended with the most unhappy consequences. The chief parts to be avoided in this operation are, the long ciliary nerve and artery, the ciliary processes, the retina and lens; if one or other of these is injured, the result is the most serious, and the intention of the operator may be completely frustrated. The patient, even before the needle has entirely penetrated the eye, often cries aloud, from the horrible pain excited, and there can be no doubt in this case, that one of the large ciliary nerves is divided. This may indeed occur to the most expert operator, where the nerves do not follow their natural course, but such instances are comparatively rare, and we may most commonly account for the accident, from the very faulty manner in which the instrument has been made to pierce the sclerotica. As these nerves run in bands along this tunic, and the external most generally shape their course directly in the equator of the eye, we should direct the needle somewhat below this. In this manner, we likewise avoid the great ciliary artery, which accompanies the nerve. These nerves and arteries anastomosing again before the ciliary band, which stretches to the distance of a line all round the cornea, they are inevitably injured too if the needle be brought to enter any where within this space. The processus ciliares may be wounded also, and the consequence is, a sudden and profuse hæmorrhagy in the posterior chamber, which completely obscures the field of the intended operation. By making the puncture about a line and a half from the cornea, and a little

distance below the horizontal diameter of the eye, we run in no danger of wounding either of these bodies, and at the same time avoid injuring the retina. The needle should be directed neither parallel nor perpendicular to the iris, but in an oblique manner, as if we intended to pierce the centre of the eye.

The German oculists distinguish two modes of couching. Depression, or the depressing of the lens immediately downwards and behind the iris; and Reclination, where this body is not only pushed downward, but backward and outward into the vitreous humour. The first is the elder operation of the two, and was much practised by a certain Willburg,* till finding it was accompanied by unhappy consequences, as hæmorrhagy, vomiting, &c. he had recourse to the latter. Professor Langenbeck, one of the most distinguished surgeons of the present time, advised, that the operation be performed per corneam; but though the surgeon is enabled by this method to see every step of his operation, he is liable to injure the iris by the neck of his instrument, and the lens is seldom or never perfectly depressed. The depression of the lens is only practicable where the cataract is very small, as in the congenital and secondary cataract.

In Reclination of the cataract, great care should be taken not to push the lens too far back into the vitreous humour. This is a very common fault with young operators. The lens in this case resting upon the retina, amaurosis is often the consequence, and the patient, who before the operation could find his own way, or distinguish larger objects by twilight, is now completely blind. Professor Beer in his lectures, relates a case of this kind, which he saw in a person who was operated upon by Mohrenheim, and where the blindness continued for eight years. His friends one day to amuse themselves, made him drunk, and in this condition, he was put to bed. Stretching himself out in the course of the night, and still labouring under the effects of the liquor, he fell from his bed, (which according to the fashion of those days, was of great height) and his friends were not a little surprised to find, the next morning, his sight again in the same condition it had been

* Abhandlung über den Staar, 1785.

in eight years before. His first exclamation upon the servant's coming into the room as usual to dress him, was, "I see light;" and pointed to the window where it entered. In this state the lens was extracted by the professor, and the patient completely recovered his vision.

Richter seems to have been well acquainted with this circumstance, and advises that the cataract be made to assume its former position, by the patient's jumping from a stool or other height, or by shaking his head violently.

A third mode of operating for cataract is, *the breaking up of the substance of the lens, and bringing it into the anterior chamber for absorption.* The term *Keratonyxis* is commonly applied by the continental writers to this operation; it is better known in England, and in this country, under the title of Saunders' operation. Without meaning, however, in the least to detract from the merits of this gentleman, we cannot but question his claims to the discovery, as the operation had been described and practised long before his time. We have only to refer to the works of Mr. Pott,* and we shall find a full and accurate detail of almost every thing that is known on the subject at the present day. To Mr. S. is justly due, however, the merit of having called the attention of surgeons to an operation, which otherwise might have been passed over with indifference or total neglect.

There are not wanting others, who have laid claim to the discovery; and it is no very easy matter to decide to whom we can

* "In order to render the fact still more clear, I have sometimes, when I have found the cataract to be of the mixed kind, not attempted depression, but have contented myself with a free laceration of the capsule; and having turned the needle round and round between my finger and thumb, within the body of the chrystalline, have left all the parts in their natural situation: in which cases I have hardly ever known them fail of dissolving so entirely as not to leave the smallest vestige of a cataract. In a few instances I have pushed the firm part through the pupil into the anterior chamber, where it has always gradually and perfectly dissolved, and disappeared, not producing pain or trouble while such dissolution was accomplishing." Chirurg. Works, vol. III. p. 156.

For some account respecting the antiquity of this operation, see Med. and Physical Journal, vol. 39, p. 136, and Journal of Foreign Medicine and Surgery, vol. 1, p. 400.

justly assign the priority. Thus, *Gleize*,* in attempting to extract a cataract, was prevented by the convulsive motions of the eye, from completing his first incision through the cornea, and withdrawing his knife, he introduced a needle through the wound, and punctured the capsule. The lens was absorbed, and the pupil again became perfectly clear. He practised this mode often afterwards, and found that in from two to six weeks, the fluid cataract was constantly absorbed. *Conradi*† adopted the practice of Gleize, but used, instead of the needle, the cataract knife to puncture the capsule. Professor Beer attempted the operation about the same time with the latter, and his experiments are detailed in the same work. The result of these, however, was most unfavourable, as in twenty-nine instances, one only perfectly succeeded. This, he now very justly attributes to the improper choice of the cases upon which he operated. Buchhorn,‡ of

* Nouvelles Observations pratiques sur les Maladies de l'oeil et leur Traitement. Paris, 1786.

† From the well known fact of the chrystalline being gradually absorbed, whether it be of a hard or soft consistence, if the capsule is opened, and the humour allowed to come into free contact with it, Conradi was led to propose the following very simple operation:—A small lancet-shaped cataract needle is introduced through the cornea, exactly as the knife in extraction, only a little further distant from the iris. The point is passed through the pupil, and when the capsule is sufficiently opened, the needle is withdrawn from the eye, which is bound loosely for two or three days, as, in general so slight a wound of the cornea is quite imperceptible, and then we wait for the absorption of the cataract. Conradi adds, it is unnecessary to dwell upon the advantages of this operation; it is much more easily performed than any other. From this very slight and trifling wound of the insensible cornea, no bad symptoms are to be dreaded, which partly occur during extraction and depression, partly follow after these operations. If after eight or twelve weeks, the cataract is not absorbed, any other operation may be performed as easily as if this puncture had not been made. The patients have lost nothing but the time; and as they have in general been many years blind, it is not of much consequence, a few weeks more or less, in order to make the trial of freeing them from their disease by a safe and easy method." Arnemann's Magazin fuer die Wundarzneykunst, I band, p. 59, or the London Medical and Physical Journal, vol. 30, p. 71.

‡ De Keratonyxide disser. inaugur. Halae 1806. Eine neue gefahrlosere Methode den grauen Staar zu operiren. Von W. H. J. Buchhorn, Magdeburg, 1811.

Buchhorn was the first to give the name of Keratonyxis to the operation.

Magdeburg, afterwards published a dissertation on this subject, in which he recommends the laying aside of the knife, and operating entirely with the needle. He gives the preference to the method of Conradi, as the lens is more liable to be absorbed, when the capsule is freely lacerated with a needle than where it is simply divided with a knife. To prevent the escape of the aqueous humour, Buchhorn made use of a round needle, slightly curved at its point after the manner of Scarpa, and which became gradually thicker towards the handle, so as to fill up completely the puncture of the cornea. It is evident that this form of instrument must penetrate with great difficulty the cornea. Professor Langenbeck*, of Goëttingen, to do away this objection, adopted the usual couching needle of Scarpa, and operated, as he says, very successfully upon both hard and soft cataracts. He experienced not the least inconvenience from the escape of the aqueous humour, and the consequent collapse of the iris, two circumstances so much dreaded by Buchhorn. This needle too, was not without its disadvantages: it would occupy too much of our time, however, to dwell upon the various improvements made upon it by Langenbeck, Græfe, Walther, Benedict and others. Many oculists, and Beer among the number, give the preference to a strait needle, with a spear-shaped point; and this has certainly the advantage of penetrating much more easily the cornea, than an instrument with a curved point. The surgeon can likewise direct its motions with much greater precision.

A few hours previous to the operation, it is customary to drop into the eye, a small quantity of the infusion of belladonna, (one scruple of the extract to an ounce of distilled water) for the purpose of dilating the pupil. This may be repeated again a very short time before the operation. If there is no adhesion between the capsule and iris, the latter will be scarcely perceptible, or is seen to perform a narrow ring only within the circumference of the cornea. The whole of the anterior surface of the cataract is in this manner exposed to view. The puncture may be made upon the external or lower side of the cornea. Langenbeck pre-

* Prüfung der Keratonyxis, Goëttingen 1811, p. 31.

fers the latter mode, as in this manner he is enabled to operate with the right hand on either eye. Having penetrated the cornea, the needle is to be carried forward parallel to the plane of the iris, until it reaches the centre of the pupil, when its point is to be dipped inwards towards the lens. By a gentle motion of the instrument from side to side, we endeavour to destroy as much of the anterior capsule as fills up the natural pupil; and having accomplished this, we proceed to break up the texture of the lens itself. Great care should be taken that no filaments of the capsule remain to intersect the pupil, as they become so elastic as to resist every effort afterwards made to depress or break them: and so long as they preserve their connection with the sound parts, they can never be absorbed. It is not sufficient either for the destruction or absorption of the lens, that its texture alone be broken, but as many of the fragments as possible, should be brought into the anterior chamber.

The time occupied by the absorbents in taking up these fragments, varies according to the consistency of the cataract, and the age and constitution of the patient. If he be old or decrepid, the operation will be most liable to fail, as the power of absorption is in such cases but very feeble. It often happens that the absorption, though it go on very rapidly for a time, is suddenly arrested. The dropping into the eye a little of an infusion of the hyosciamus, or pencilling it with Mr. Ware's solution of opium, where no inflammation is present, will be found to answer a very good purpose.*

Another mode of performing keratonyxis, is by passing the needle through the sclerotica *behind* the iris, hence, this has been called the posterior operation. "Few cases of lenticular cataract occur to which this is appropriate. There is more pain, more

* Not only the lens, but also foreign bodies when lodged in the anterior chamber undergo this process of absorption. Thus Mr. Cline, in operating for cataract accidentally broke the point of his knife, which was deposited in the eye. It could be distinctly seen afterwards, glistening through the cornea. It became rapidly oxidated, and the whole chamber was rendered turbid. In a few days it was completely absorbed, and the eye resumed its natural appearance.

inflammation, more danger of displacing the lens, than in the operation through the cornea. But when the capsule is opaque, and the lens diminished in bulk either spontaneously, or in consequence of former operations, so as to have receded from the pupil, the posterior operation is more eligible, as the operator readily ascertains the effect of the needle upon the capsule, and directs its movements to the best advantage, while the inflammation is always moderate in proportion as the lens is small. Indeed, in the purely capsular cataract of the adult, and the half absorbed cataract of children, it is so slight as scarcely to confine the patient. When a cataract of firm consistence has undergone a partial absorption from one or more operations with the needle, and still does not readily separate into fragments, the introduction of the needle posterior to the iris gives the surgeon the advantage of couching it. This I have often done to the great satisfaction of the patient, who escapes owing to the diminished bulk of the lens, the inflammation which occasionally follows the primary operation of couching."*

The operation of keratonyxis has several advantages over those of extraction and couching.

1. A single coat of the eye only is injured: and as this contains neither large nerves nor blood-vessels, there is no danger of hæmorrhagy, and the pain occasioned is so slight, that the patient is easily reconciled to undergo this operation, when he will refuse to submit to any other. It is on this account that we would especially recommend the operation in children, where the pain from piercing the sclerotica is often so violent as to cause convulsions.

2. We may better control the motions of the eye, particularly if the needle be introduced from below; as the eye-ball is much more apt to recede towards the root of the nose, than the orbitary ridge, when threatened by the needle.

3. From the transparency of the cornea, we are enabled to observe every motion of our instrument, and to direct the operation with the more precision. The operator's attention, besides, is confined to the manipulations of a single instrument, and not

* Travers on Cataract. Vid. Medico Chirugical Transactions, vol. 4, p. 292.

divided as in extraction and couching among a variety of difficulties.

4. The surgeon is enabled to operate with the right hand on either eye; and this is no small advantage to the young and inexperienced operator.

5. The cornea having no connection with the iris, wounds of the former never affect the latter. Hence iritis is seldom a sequela of the operation, unless the iris have suffered from the awkward manner of using the needle, or from adhesion between this membrane and the capsule of the lens. Professor Langenbeck performed the operation twenty-eight times, and in one instance only, was it followed by inflammation of the iris. The inflammation in general is confined to the conjunctiva, and yields very readily to low diet, formentations of tepid water, and purgatives.

6. If the operation be properly performed, the anterior capsule is always destroyed, and one of the most frequent causes of secondary cataract thus avoided.

7. The operation may be performed on young children and infants as readily as upon the adult, as from the dilated pupil, there is little danger of wounding any important part of the eye.

8. The operation may be repeated any number of times without injury. It is on this account, that the surgeon should avoid the too free use of the needle in his first operation. He is the more apt to commit this error, from a fear that the patient's fortitude will not hold out to have a second performed.

9. The posterior capsule, the central artery of the retina and vitreous humour, are not in the slightest degree injured by this operation, and hence amaurosis and a total wasting of the eye, which are so often the consequence of division through the sclerotica, are seldom or never found to follow division through the cornea.

The operation has of late got into much disrepute, owing merely to the indiscriminate manner in which it has been adopted in all cases of cataract. M. Demours, in his very splendid work, has devoted scarcely half a page to the subject, and condemns the operation in toto; and from the attempts lately made by M. Roux of Paris, to revive the operation of extraction, we dare hardly hope

to see its claims again brought forward. It is due to Mr. Saunders to state, that its superiority was distinctly asserted by him in cases of soft and capsular cataract only. He intended ascertaining the results in the other varieties, but was prevented by an untimely death. Mr. Travers, his pupil and successor, followed up his intentions, but his success was little satisfactory. He says, " with every precaution, the operation with the needle upon the full and firm cataract, is either distressingly tedious, or what is more to be objected to, destructive to the organ, and very trying to the health and spirits of the patient. In such cases, I have therefore relinquished the operation, which was very ingeniously contrived, and for its simplicity appeared to me to deserve a full and fair trial of its merits."* The reasons for its failure are given in a previous page of the same paper.

Having now gone through some of the chief points which relate to the different operations of Extraction, Couching and Division of the Lens, it remains for us to notice cursorily certain circumstances of the disease itself, which render the one or other of these modes of operating most eligible. It is evident from the great and complicated variety we have noticed in cataract, that no precise or established rules can be laid down upon this subject; but that much must be left to the judgment, discrimination and experience of the surgeon, in determining the choice of his operation. It is in vain that certain writers would attempt to persuade us that either of these modes can be adopted indiscriminately in all cases, or that by any newly invented or improved method, any single operation may be rendered applicable to all the varieties of cataract. The merest tyro in the profession must at once perceive, that it were absurd to talk of extracting a soft cataract, dividing a hard, or depressing a large one; yet the most skilful and able surgeon will often feel himself at a loss, even where the diagnosis is sufficiently made out, in selecting between the one or the other of these methods.

We may lay it down as a general rule to extract in all such cases, where the cataract occurs in a healthy subject, without any

* Travers, Op. Cit. p. 291.

complication of other disease, and is of the species we have denominated dura, where the eye remains firm and quiet under the knife, where the pupil is large and dilated, and where the anterior chamber is large and full. These are upon the whole the most favourable cases for any operation.

The extraction of the lens, on the contrary, should never be resorted to in those cases, where the lens adheres in some part of its surface to the iris, where the cornea is very flat, and the anterior chamber of the eye of course so much diminished, that the section of the cornea cannot be made sufficiently large to admit of the passage of the lens, where this tunic presents the appearance, denominated arcus senilis, as the wound made under these circumstances never heals, where there is present an habitual contraction of the pupil (myosis), where the eyes are seated deep within their sockets and the eye-lids cannot be opened to a sufficient distance; where adhesion has formed between the iris and cornea, where the cornea is affected by maculæ or other diseases, where the patient is very timid and irritable, or old and cachectic, and where any particular predisposition prevails to inflammation of the eye, chronic ophthalmia of the lids, &c. In all such instances, couching, or the division of the lens are to be preferred, and wherever the cataract be sufficiently soft to admit of absorption, the division is always the most advisable and safe of the two operations.

Art. IV. *Some Cases and Observations on Dyspepsia.*—By John Revere, M. D. of Baltimore.

"I doubt not but that every one will, on reflection, consider the disorders of the digestive organs to be of the first importance, and will perceive the propriety of diligently inquiring into their nature, that we may know them when they exist, and that our attempts to remedy them may be conducted on rational principles." Abernethy.

It is a benevolent, as well as wise provision of nature, that taking food should at once impart delight, and renovate the living powers of animals. In man, while this gratification is enjoyed within the bounds of reason, it invigorates his health and

is at the same time an abundant source of enjoyment. But, as man excels the other inhabitants of the earth in intellectual endowments, he is too apt to employ these powers in endeavouring likewise to surpass them in sensual gratification. His power in this respect, however, is exceedingly limited. An excessive use of any of the organs of his body is followed by a proportional reduction of its living powers, during which its functions are imperfectly performed, and fail to impart the usual degree of pleasure. This is one of the many means by which the apparent disparity in the condition of men is, in fact, equalized.

There is no function of the living animal body, the phenomena of which have been more satisfactorily explained than those of digestion, particularly during its first stage. But, although the physiology of the stomach has been so successfully investigated, the application of this knowledge to the pathology of the organ has been unaccountably neglected.

Dyspepsia is spoken of by elementary writers, for the most part, as arising from "an imbecility, loss of tone, and weaker action in the muscular fibres of the stomach," which alkaline and tonic medicines will relieve. But observation will shew, that this is a narrow view of the nature of the disease, that a derangement of the functions of the stomach is often attended with the most anomalous symptoms, that it is protracted in its continuance, and absolutely ungovernable, if combatted by the remedies, and according to the rules which these writers lay down. Being under an impression that many cases occur in practice in which symptoms, arising from derangement of the stomach, *affect* the characters of other diseases, and in which, from sufficient emphasis not having been laid on this subject, the mind of the practitioner is apt to be misled, I have ventured to present for the consideration of the profession the following cases and observations.

Though no age or class of society is entirely exempted from dyspepsia, yet it most frequently attacks those who have passed the age of 25, and persons in the higher ranks of life. The persons whom I have observed to suffer the most frequently and most severely from this complaint are those whose habits are sedentary, or whose situation and inclinations lead them to indulge most

frequently in the pleasures of the table; those who are induced to go often into company, where the hour of eating is irregular, and the desire for food increased by the lateness of the hour; where the appetite is excited by the variety of rich flavoured, and high seasoned food, and the over-distended stomach gorged with the most discordant articles of diet. If such indulgence becomes habitual, more especially if the night be substituted for the day, the almost necessary consequence, even in the most vigorous constitution, is the disease which is the subject of the present inquiry. It often makes its approach in the most gradual and insidious manner. For months, and sometimes years, the unpleasant consequences of such habits are only temporary, and easily removed by regularity and temperance. But, if they are persisted in, the restorative powers of the digestive organs become impaired. This is not sudden, nor is it generally manifested by any diminution in the desire for food. On the contrary the appetite is not unfrequently voracious. Whether this arise from the habit of distention, to which the stomach has been accustomed, or from some morbid change in the structure of the organ, or the character of its secretions, it is not, perhaps, easy to determine; the phenomenon is, however, one of frequent occurrence. The patient at first finds that the unpleasant effects of a single indiscretion are more severely felt, and require a greater length of time to get rid of them. Instead of being cured in a few hours, by the salutary efforts of nature, he is obliged, after considerable suffering, to resort to medicine and abstinence for relief. He is at first, however, rarely so unwell as to be detained from his usual routine of pleasure or business. No sooner is he relieved from his most troublesome sensations than, notwithstanding his recent sufferings, and though he may be conscious at the time that a repetition of the same causes will produce the same, or even worse, effects, yet so much has his resolution become enfeebled by the habitual indulgence of his appetite, that, pressed by a sensation of hunger, he again gives way to temptation. Perhaps at a moment when he feels a peculiar necessity for abstemiousness, and in fact is scarcely sensible at the time that he is not so, he eats freely of cakes, preserves, nuts, dried fruits, or other

articles of food improper in his situation. A restless night, troublesome dreams, excessive thirst, nightmare, distressing sensations about the scrobiculus cordis, numbness of the scalp, &c. are the consequence. The stomach, perhaps, in the course of the night, unable any longer to endure the load with which it is oppressed, by a salutary effort, relieves itself of the disgusting mass, which is found to have undergone no other than a chemical change. If this be not the case, the sufferer is awaked from his feverish and tiresome sleep, by nausea and acid eructations, savouring of the food taken the preceding evening. The bowels are either costive and flatulent, or nature endeavours to rid herself of the evil by a diarrhœa. The patient at last rises to the business of the day with a haggard countenance and a dejected mind. The alteration in his appearance, though gradual, is at last noticed by his friends. But although his form be reduced both in strength and weight, yet he is able to keep about and to be guilty of similar indiscretions. At last the assimilating powers of the digestive organs are almost lost. The stomach becomes so irritable that the patient is unable to eat the most common article of food, without producing the greatest suffering. A potatoe, a piece of cabbage, or even a piece of bread will produce the greatest uneasiness, until at last the attenuated form exhibits the appearance of a perfect marasmus. In the advanced stages of the disease, the mind is always more or less affected; in some instances very seriously.

But although the above described mode of life be a prolific, it is far from being the only source of this most distressing complaint. The stomach is evidently the principal seat of the disease. The functions which this organ performs are most important, and its sympathies, perhaps excepting the brain, more extensive and remarkable than those of any other. To one who has not been in the habit of observing accurately the phenomena of animal life, they are almost incredible. Reasoning then, *a priori*, we should anticipate, what experience proves to be true, that any thing which tends to reduce the vital energy, or disorder any of the various functions of the body, would occasionally be found to produce this disorder. Long continued anxiety of mind, sedentary

habits, the infirmities of age, general and local, acute and chronic diseases, in all their multiplied and varied forms, are often the remote causes of this disease, even when the stomach might be supposed to be less concerned than many other parts. On the other hand, it not unfrequently happens, that a train of painful and distressing symptoms arise, with which the stomach seems at first remotely connected, when in fact some affection of this organ is the primary and essential cause of the difficulty; though this is not suspected by the physician, whose mind is apt to be misled by hypothetical reasoning, and nosological distinctions.* Persons recovering from acute diseases, and in certain peculiar conditions of the body, will sometimes suffer severely from some slight impropriety in the quality of the food, or some trifling irregularity in the period of taking it. On these occasions it often happens that the causes seem so entirely disproportioned to the effects produced, that the circumstances are not mentioned by the patient, without close questioning, or, if they are, they seem too insignificant to attract the attention of the practitioner.

There can be no doubt that this disease may be found wherever civilized society exists. It is, however, a fact well ascertained, that in some portions of this country, it is productive of more serious effects, if not of much more frequent occurrence, than in others. In Massachusetts, particularly at Boston, it has of late prevailed to a very alarming extent. The number of individuals, who have been affected by it, seems to have been considerable; though, from the rank that many of them hold in society, and from their being in the habit of travelling, they may appear more numerous than is actually the case. Nevertheless, it is certain that the disease has exhibited itself in Boston, and its neighbourhood, in a more ungovernable and terrific form, than in any of the other large cities in the United States. Dr. Mead, in his account of the mineral waters of Saratoga and Ballston, speaking of the character of this disease in Boston, remarks:—"It soon reduced the patient to a state of the utmost debility and emaciation, such as is

* For some very valuable cases and observations on this subject, the reader is refered to Abernethy, "On the constitutional Origin and Treatment of Local Diseases."

usual in the last stage of consumption; the countenance is altered, the cheeks are prominent, the eyes look hollow and languid, the hair often falls off, the nails are of a livid colour, and the pulse, sometimes, becomes so slow that I have, in one instance, been able to count but 45 strokes in a minute." There have been certainly some cases where the disease goes to this extent. Dr. Mead supposes them to arise from a scrofulous diathesis, and to be in fact a species of marasmus, and not dyspepsia. In support of this opinion he undertakes to assert, that "it principally attacks persons at a very early period of life," that the progress of the disease, the physical conformation of the subjects of it, and the remedies successfully employed are precisely those of scrofula. I am satisfied however that the Doctor is mistaken in each of these points. From personal observation, I am compelled to believe, that the disease, described by Dr Mead,* "does not make its attacks at a very early period of life, but generally in subjects upwards of 25 years of age. That it is not *aggravated in the winter months*, but is usually mitigated in these months and troublesome in hot weather. That the persons who have had this disorder, *have not been remarkable for any particular formation.*" But, "that it arises from the same causes as dyspepsia, exists in the same kind of subjects; exhibits the same symptoms, and is cured by the same means, and that it is neither more nor less than dyspepsia."

Whenever the power of the digestive organs is reduced to the state, which has been above described, whether it be the consequence of a long continued system of indulgence, sedentary habits, or the direct result of other diseases, the reception of food into the stomach, which does not readily run through the process of digestion, produces often the most distressing and anomalous symptoms. I will now relate some cases which, I think, will shew that, under these circumstances, eating small quantities of food, which in the healthy state of the stomach is perfectly innocent and nutritious, is sometimes followed by nausea and vomiting, great general distress, sense of numbness in the scalp, violent colic, acute pain in the side and bladder, vertigo, apoplexy,

* N. E. Journal of Med. and Surgery, No. IV. Vol. 6.

and perhaps convulsions and death, and that, if the indications of cure *seeming* to arise from these symptoms be pursued, there is reason to apprehend that the most serious consequences may ensue.

CASE I.

In the winter of 1812-13, a gentleman was compelled, under great anxiety of mind, to spend several weeks in travelling. It was not until after great exertions and perplexity that he was enabled to accomplish his purpose. As soon as his attention became less engaged in his business, and he had time to contemplate his own feelings, he found his health impaired. He was, at the period of which we are now speaking, about 35 years of age, of a robust form of body inclining to corpulence. He had resided much abroad, and though perfectly temperate in his habits, had lived luxuriously. Previous to this time he had considered himself a healthy man, and was in appearance eminently so. He had been however for some time subject to frequent attacks of constipation, flatulence, &c. which a brisk cathartic and a little abstemiousness would remove, but this had not excited much attention. On his return he found his health so seriously impaired that he was under the necessity of applying for medical aid. He was considerably emaciated, his bowels were constantly costive, and he had had no stool for some time, without the employment of cathartic medicine. This however, he attributed to travelling, which he had frequently observed to produce this effect. He had a keen relish for his food, and, for an hour after eating, he felt the better for it, but, after that period, he was often attacked by a variety of distressing sensations. Sometimes it was an acute pain in the stomach, and a sensation as if that organ was distended with flatus, &c. at other times, after eating what he considered a very light supper, he would be awoke by a numbness of the extremities or the scalp. The latter was a most distressing sensation, and one to which he was particularly subject. As soon as he awoke, he was obliged to jump out of bed, and have recourse to the flesh brush, which by a brisk friction, would relieve him. But the most obstinate and troublesome symptoms were, acidity

of the stomach, and a constantly torpid state of the bowels. His physician had recourse to the usual routine of remedies employed in such cases. Emetic, cathartic, alkaline and tonic medicines, &c. were occasionally employed, and sometimes, afforded temporary relief. The disease however was evidently gaining ground. His temper became irritable, and was so easily ruffled, that trifling accidents produced the most serious unhappiness. From being a corpulent man, with every mark of vigour and health, he became as pale and emaciated, as a person in the last stage of phthisis pulmonalis, a mere walking skeleton, hardly recognized by his most intimate friends.

Various medical advice was had recourse to, and as various remedies employed. The most efficient, however, was the exhibition of calomel until ptyalism was produced. This mode of treatment was fairly tested by keeping the system under the influence of this agent for several weeks, but with little or no benefit. For several months after the discontinuance of the mercury, but little recourse was had to medical assistance. The disease was mainly attributed to some organic lesion, for the removal of which, medicine was supposed to promise but little benefit, and radical relief was almost despaired of. After great suffering the gentleman placed himself under the care of a physician who had not heretofore been consulted in the case. After the exhibition of a smart emetic, the cure was chiefly trusted to the following diet and regimen. The diet was directed to consist chiefly of animal food, and to be taken at regular periods. The breakfast a single cup of coffee, with but little sugar or milk, and a small piece of beef-steak rare, with no other dressing than the juice of the meat. The dinner consisted of the lean part of meats, either roasted or boiled, in moderate quantity, and, of these, beef, mutton, and game, were preferred, with no artificial gravies, and no vegetables but onions, and unfermented bread. The drink with the dinner either a half glass of brandy and water, or two glasses of Madeira wine. During the intervals between meals, the patient was advised to take as little fluid as possible, and that pure water. After dinner no food was allowed. The body, though it was then summer, was covered with flannel, and frictions employed for half an hour, to its whole surface, daily before dressing, and warm

bathing two or three times a week. To remove the habitual constipation the patient was directed to make an effort to go to stool at a regular period, to ride briskly on horseback before eating, and if absolutely necessary to employ enemata.

The effect of this system was most satisfactory. For months his sufferings had been almost incredible, until life had become a burden. From the period of adopting this diet and regimen an amelioration of his sufferings took place, his health and appearance gradually improved. I saw the patient last summer, and though he is still thin, and any indiscretion in diet is still apt to remind him of his former suffering, yet he may be considered, comparatively speaking, a pretty healthy man.

CASE II.

In the autumn of the year 1815, a lady, then aged about 25 years, and who had enjoyed an unusual share of health, was confined with her third child. She had a short illness and got over it well, and her convalescence was rapid. At the end of a fortnight she had apparently recovered her usual share of health. About this time, one night while in a sound sleep, she was awoke by the cries of one of her children who slept in an adjoining room. Without being conscious of what she was about, she sprung out of bed and ran into the adjoining entry, where she stood for a moment with her naked feet on a painted floor. She was immediately reminded of the impropriety of remaining in this situation, and returned to her bed, but not until she was exceedingly chilled. In the course of an hour afterwards she was seized with pain in the abdomen, which increased rapidly, and when I saw her, which was soon after, the pain about the umbilicus was so great that she could not refrain from shrieking with agony. The hands were cold, and the pulse so feeble that it could scarcely be felt at the wrist. Various warm applications to the abdomen, sinapisms, &c. together with laudanum, in large quantities, were had recourse to for the purpose of obtaining, at least, a temporary relief. After some time, the laudanum was rejected by vomiting, but the violence of the pain became in a great degree mitigated, and by using enemata and cathartic

medicines, the alimentary canal was emptied of its contents, and there seemed every reason to hope that the evil was radically removed. The result, however, showed this expectation to be unfounded. The patient was confined to her bed for several weeks with peritoneal inflammation, and its sequel, by which she lost her milk and nearly her life. Her convalescence was protracted by the prostrated state of the stomach. For many months after this, mental agitation, or the slightest indiscretion in diet would be followed by the most dreadful paroxysms of suffering that I have ever witnessed. Or, if after eating any indigestible food, she exposed herself to cold, or wet, she was sure to bring them on. She had notice of the approaching evil, by a dull pain and sense of soreness in the back, which gradually extended round to the stomach, which, for the most part, came on several hours after eating. This would steadily, and sometimes rapidly, increase until the disease seemed to concentrate all its violence about the pit of the stomach, or the navel, where it would remain for hours producing a degree of pain amounting to agony. During the violence of the attack the extremities were cold, shrunk, and livid, and the face almost Hippocratic. The action of the heart and arteries was frequent and feeble, and, as often happens in colic, the stomach was irritable, rejecting every thing which was put into it. The bowels were generally costive, which was the natural habit of the patient. The pain was so exquisite that the first and most urgent indication was its removal. For this purpose, and to produce some reaction, the most powerful antispasmodic and stimulant remedies were at first employed. Brandy and water given hot, peppermint and water, laudanum and æther, in the largest doses, assisted by external applications of sinapisms to the extremities and scrobiculus cordis, were repeatedly tried without producing any reaction in the system, or even mitigating the pain. The intention was, to subdue at first the most distressing symptoms, by the opium, and to depend for radical relief upon active catharsis. I have never known this practice, when pursued boldly and steadily, in any other subject to fail. In this, however, after having given it a fair trial in three paroxysms, and finding it entirely impotent, I abandoned it. In several

preceeding paroxysms, I had recourse to cathartics, enemata, &c. but I did not find much more reason to be satisfied with this than the former mode of treatment. The cathartic medicine, of whatever kind, and however given, generally remained but a short time without being rejected by the stomach, or if it remained, it seemed to have little other effect than to increase the pain. Having at last observed that the intervals between the violent pain were longer and more complete after every effort to vomit, and the patient having made the same remark, I was induced to try, at the commencement of one of these attacks, an emetic, though there seemed every reason to believe that the paroxysm was brought on by exposure, and that indigestible aliment had no connection with the disease. The relief was prompt and effectual, the fit was completely cut short, after copious vomiting consisting chiefly of the secretions of the stomach and neighbouring organs. It is now four years since this experiment was first tried, previous to which the patient had attacks, which began and terminated in the manner above described, on an average at least one in two months, from which she seldom fairly recovered in less than a week. Since that time she has had but two paroxysms, which I have not been able completely to arrest in the course of an hour and a half, by the exhibition of an emetic as soon as the approach of the disease is distinctly perceived. She has quite recovered her strength, and flesh, and spirits, and may be considered a healthy woman.

This is one of those cases, which I presume every candid physician will acknowledge to have occasionally encountered, where the judgment was misled by preconceived opinion, and hypothetical reasoning. Though I was actuated by the most powerful motives, to make every effort to find out the best method of treating this case, and though I was sensible that the stomach had considerable agency in producing the disease, yet the peculiar circumstances were such that I confess I was completely mistaken in its treatment in the early period of its history. It was a lesson which I was slow in learning, but which I trust I shall not easily forget. I was misled in the first place by observing that the paroxysm frequently took place several hours after eating,

and when the food was often not of an indigestible kind. It seems probable, from the history of this case, that after eating food, by no means indigestible in itself, if the body was exposed to a sudden change of temperature, or any other powerful impression, arising from agitation of mind, &c. &c. the process of digestion was immediately suspended. The half digested aliment which remained in the stomach, though often very small in quantity, acting upon the organ, in the state it then was, produced the series of painful symptoms which have been described. I was likewise deceived by finding that, though the pain began in the back, it gradually extended to the umbilicus, while the stomach often remained free from pain. Instead of expecting the approaches of this formidable disease with dread, marking its progress with the most harassing solicitude, and looking forward to its termination with the doubtful anxiety, which my frequent and painful experience compelled me to feel, I now meet it with a confidence that I have in my power the means of rendering impotent its most painful attacks.

CASE III.

In December 1818, I was requested to visit a gentleman who was a stranger in this city, aged about 50 years. He sent for me in haste quite early in the morning, and was then labouring under a very acute pain in the left side under the sixth rib. He had taken a long journey, and had been in the city about a week. During the latter part of his journey, he observed that his appetite had become impaired, but that he had not suffered any unpleasant sensations worth mentioning, after eating. Although he had a stool every day, yet the quantity of the excretions did not seem to bear a proper proportion to the ingesta, and for several days, he had felt his bowels swelled, being naturally a corpulent man. In order to induce a more open state of the bowels, he had been in the habit of late of eating a greater quantity of vegetable food than usual, and particularly he had eaten very freely of corn mush, (a soft pudding made of Indian meal.) Three days before my seeing him, feeling a necessity for some cathartic medicine, he had taken three of Lee's Bilious Pills, a medicine he had

frequently used, and which in this dose had always operated freely upon him, but on the present occasion the operation had been very imperfect. For the two nights preceding my visit, he went to bed tolerably well, and slept quietly the greater part of the night, but about day light he was awoke by a dull pain in the side in the spot already mentioned. This pain increased in severity, until it became extremely acute, and was attended by a sensation about the rectum and bladder as if these organs were distended with their usual contents, and with a most urgent desire of discharging them; the effort was sometimes ineffectual, but even when successful, did not produce any essential mitigation of his sufferings. The paroxysm continued from an hour and a half to two hours, after which the pain gradually disappeared, leaving him in a profuse perspiration. This was the third time he had been taken in this way. At the time I saw him he supposed its greatest severity to be over, the pulse was 92, and the skin growing moist, the tongue healthy in its appearance, on making a deep inspiration the pain in the side was not increased, nor was there any febrile irritation. He had taken for supper, tea, and hot cakes made of Indian meal. On questioning him he assured me that he had not experienced any uneasy sensation about the stomach during either of the paroxysms, and he seemed very incredulous when I told him I suspected that this organ was the seat of the disease. I prescribed a smart cathartic of rhubarb and calomel, to be followed up with a diet similar to that mentioned in case 1st. The medicine operated sufficiently, he spent the day and night well, and had no paroxysm on the following morning. Feeling very well in the morning, and thinking perhaps my directions as to his diet unnecessarily rigid, he ate for his breakfast hot corn cake of which he was fond. In the course of an hour afterwards, he was seized with an unusually violent paroxysm, which was succeeded by such debility as kept him at home all day. Being now satisfied that my first opinion was right, he became very attentive to his diet. He had no farther return of the pain, but was restored to health in a short time, without taking any more medicine except a little ærated magnesia water to keep the bowels open.

CASE IV.

A lady aged about 65 years with a constitution completely broken down, and afflicted with all the usual infirmities of age, in the month of October 1818, was attacked with diarrhœa, attended with tenesmus. This was a complaint to which she was very subject. It was readily relieved, so that on the third day she seemed nearly well. On all similar occasions I had remarked that the stomach became very irritable, and at this time its functions were so much deranged that I gave very particular directions respecting her diet, an attention to which seemed all that was wanting to restore her to her usual state of health. I had ceased to visit her, and she continued rapidly to recover for two days, but, on the third day, which was Sunday, the servant called and left a message requiring my attendance. As I had no reason, from the message, to suppose the case an urgent one, I remained for nearly half an hour conversing with a friend, and then went leisurely to the house, which was at a considerable distance from my own. When I arrived, I learnt that she had remained very well since I last saw her, until since dinner. She had eaten her dinner with an excellent appetite, which had induced her to eat more than usual. Her dinner consisted of the breast of a chicken with turnips and gravy, with bread and a little wine, and from all accounts, not large in quantity. Soon after dinner she complained of oppression at the pit of the stomach, and was then seized with chilliness, attended with great prostration of the muscular strength. Her attendants placed her on the bed, gave her a little wine, and applied friction to the extremities, but finding her beginning to grow stupid, and to answer confusedly, they sent off for me. The reason of my not arriving sooner has been already explained. I found her insensible and incapable of being roused, she was unable to articulate, her breathing was stertorous, her pulse about 70, small and weak, the countenance shrunk, and the eye without expression and glassy. Conceiving, from the history of the case, and my knowledge of the constitution of the patient, that the whole system was sympathizing with the oppressed state of the stomach, I was desirous of exhibiting an active emetic at

once, foreseeing the case must soon terminate in death, or at least in a partial relief. There was nothing in the house which would answer the purpose of an emetic, and, as the shops were shut (it being Sunday) I was compelled to return to my own house for this purpose. With all the haste I could make, it was nearly an hour and a half after she had become speechless, before I was able to get down about 30 grs. of the sulphat of zinc dissolved in a gill of warm water. She swallowed about half this quantity at a time, and with great difficulty. In about 30 minutes after she had taken the whole she vomited slightly. The effect was perceptibly beneficial, and though she was still unable to articulate, the countenance became more intelligent, and the respiration better. I then gave her about 20 grains more, with about a tea-spoon full of the powdered ipecac. mixed with it. This, in half an hour, produced copious vomiting. The whole dinner was thrown up, completely unchanged, the meat, owing to the badness of her teeth, being but little masticated. Immediately after the vomiting took place, she was astonishingly relieved. Her speech and reason returned, like that of a person suddenly awoke out of a profound sleep. In the course of two hours after the first exhibition of the emetic she was freed from every alarming symptom. She was very careful in her diet, and was soon restored to her usual share of health.

She remained free from any serious indisposition until July 1819, when she went to pass a few weeks in the country, about four miles on the York road. In consequence of a diarrhœa, she was induced to take an ounce of castor oil, which operated very freely. The next morning she eat, what she considered a moderate breakfast, consisting of bread and butter, and a cup of coffee. Soon after she complained of a disagreeable sensation about the scrobiculus cordis. This sensation continued to increase for two hours, at the end of which she was seized with a chill, and in a short time she became completely speechless and insensible. After some delay a servant was sent for me, but as a horse could not be readily procured, he was obliged to walk into town. From these delays, it was past 2 o'clock, before I saw the patient, and she was then lying in a perfect state of stupor, the respiration stertorous, and the pulse 68, rather weak

and oppressed, the eye dull and without expression, and the pupil dilated, and contracting but slightly on exposure to the light. On being shook and spoken to, she muttered incoherently for a moment, and then sunk into the same state as before. She had been in this situation since 10 o'clock, more than four hours. From the circumstances of the case, and from the good effects of an emetic, on the occasion already mentioned, I was induced to give a solution of the sulph zinc. 30 grs. with a tea spoonful of pulv. ipecac. The medicine was at last got down, but with great difficulty. In the course of an hour afterwards, copious emesis took place, the stupor and every other alarming symptom disappeared, and I left her in the course of two hours with her speech and senses restored, in fact comparatively well.

On reviewing this case one is struck with the powerful sympathies which exist between the stomach and every other part of the body, with the great apparent disproportion between the cause and effects produced, as well as the prompt relief afforded by the emetic. One can scarcely avoid speculating on what might have been the termination of this case, if the usual practice in apoplexy, v. s. had been employed on this superannuated subject.

CASE V.

It was shortly after the first attack, above described, occurred (Nov. 1818.) that I saw for the first time, being called by accident while the patient was in a fit, the following case: The subject was a gentleman aged about 35, of an athletic form, with a large head and short neck, and of a constitution naturally excellent. In the early part of his life he had spent several years abroad and mixed much with society. From his rank, numerous connections, and convivial disposition, he was induced to indulge frequently in dinner and supper parties. He was remarkable for his absence of mind. This was often observed and mentioned by his friends in his manner of eating. If he got engaged in conversation, he did not seem to notice the quantity or quality of his food. He would frequently eat the most discordant articles of diet, without seeming to be aware of what he was about. For

two years preceeding his last indisposition he had been sensible that the powers of his stomach were essentially impaired. He was frequently troubled after eating heartily, by headach, vertigo, and an indisposition to make any mental effort. He observed at these times, that his bowels were costive, and his stomach oppressed by his food. On other occasions, these sensations would be succeeded by a diarrhœa, and he often observed that his food, particularly vegetables, was passed off unchanged by the digestive organs. These attacks, although such as to make him uncomfortable, and to cause a change in his appearance, perceptible to many of his friends, were not sufficient to keep him at home, or to induce him to make a business of getting well. When his sufferings were so great that he could not any longer endure them, he was in the habit of applying for medical advice, and an active cathartic, followed by abstinence, would procure temporary relief, but he would in a short time relapse into the same situation. It was more than a year since these symptoms had, on one occasion, assumed a more aggravated and alarming character. His family were in the country, and he had been dining with a friend; as he was walking near his house he complained of feeling unwell, and suddenly fell down senseless. His friend conveyed him with assistance to his house. He was not seen at the time by a physician, and it was not easy to obtain afterwards, from common observers, the precise character of his indisposition. There seems, however, good reason to believe that the attack was similar to those which I shall have occasion hereafter to describe. A small cathartic was employed, he felt better the next day, and was able to walk out, it was called a fainting fit, and the thing almost forgotten. He continued to live along in this way, sometimes tolerably well and at others suffering severely, though never confined to his house.

During July and August, 1818, he took a long journey, and he was much in company. During this visit he was several times indisposed, though not enough to consider himself seriously sick. Finding his illness more distressing after his return to Baltimore, in the latter part of September, he again had recourse to medical assistance. After taking some pills which operated powerfully

as an emetico-cathartic, he then began upon what was deemed a suitable diet. This was mush and milk, and soups. One evening, soon after taking his supper of mush and milk, he complained of feeling unwell, and soon fell senseless; all the muscles of the body were violently convulsed, his features were distorted, he frothed at his mouth, uttering at the same time the most piercing shrieks. He remained in this situation, for about ten minutes, after which he gradually recovered his reason, of which he was evidently deprived during the paroxysm. Physicians were sent for, copious venesection was had recourse to, and a cathartic given, and in a few days every alarming symptom vanished. A similar diet to the one before employed was again directed, with this exception, that a few oysters were allowed. Every thing was now supposed to promise well, and the patient flattered himself with the expectation that he should soon be able to return to his business. But suddenly these hopes were disappointed. On the fifth day after the first attack, about 2 o'clock, a second violent convulsion took place, in character nearly like the first; several physicians were called, but none arrived until just as the convulsions ceased. On coming to himself the first exclamation of the patient was, " Doctor, (addressing the attending physician) I have had another turn," his countenance at the same time being flushed, and indicating the greatest anxiety and distress. He was silent a moment, and then laying both hands on the pit of the stomach, he exclaimed, " Doctor I have a most dreadful sensation here." On inquiring if any food had been recently taken, it was answered, a little soup and a few oysters. An emetic was proposed, but this was considered inexpedient, the lancet being deemed most adequate to every useful indication; the countenance still exhibited strong signs of the greatest distress. Copious venesection was then employed by the attending physician, who soon after went away, promising in a short time to return. He did return in about half an hour, and was setting by the bed side, congratulating himself and the friends on the flattering appearances of the case, when, suddenly, the patient was again seized with a paroxysm more violent and frightful than the first; the convulsions were terrific, and the screams such as to alarm the neighbourhood. A

consultation with a gentleman, eminent for his experience and high medical attainments, was now held, and the plan pursued was, copious depletion both from the arm and temples by cupping, and this to be repeated whenever the pulse indicated much increase of vigour in the system, at the same time the submur. hydr. was given with an intention of producing ptyalism. The diet directed was mush and molasses, mush and milk, crackers and molasses, and mutton soup; not to be given at any fixed period, but whenever the patient was disposed to take it, day or night, and the state of the bowels was carefully attended to. During this time the patient was troubled with costiveness, and excessive flatulence; sometimes he would feel an acute pain in the left breast, and this would be relieved by a discharge of flatus per anum. During the whole progress of the disease, thus far, there had been no fever, nor any other complaint in the interval between the convulsions, excepting costiveness and *extreme* flatulence, disagreable sensations at times at the pit of the stomach, headach and pain in the left breast. He was, however, greatly reduced in strength, so as to be generally confined to his bed; but this was obviously the result of the course of remedies which it was deemed expedient to employ.

While under the treatment which has been just described, when the mouth was beginning to shew that the system felt the influence of the mercury, and when a speedy and favourable termination of the case was confidently anticipated, a recurrence of the fits took place! The evidence that the brain was the seat of the disease was now considered so strong, as to be no longer equivocal, and on this presumption, besides continuing copious depletion from the arm and temple, the head was shaved and repeated blisters applied to this part. Another deceitful calm ensued, the same diet and the mercury continued. Again the hopes of the friends were disappointed, and the belief that the brain was the principal seat of the disease was confirmed in the minds of the physicians. It may not be improper here to remark, that during the two last paroxysms, the convulsions and struggles were thought to be not quite so violent as at first. By some this was attributed to the disease being somewhat subdued, but

it was supposed by others that, if in fact there was any difference, that this was more probably owing to the great reduction in the general powers of the system, than to any essential diminution in the disease itself. The case had now assumed an aspect, most alarming to the medical attendants, and discouraging to the friends. By the physicians it was considered highly dangerous, in whatever point of view it was contemplated, whether it originated in a disease of the stomach or brain, because it was supposed that the practice had been such as ought to have arrested it, in either case. Under these circumstances further medical advice was desired by the friends. On inquiring fully into the whole history of the case the gentleman consulted was induced to form a different opinion respecting the origin, and primary seat of the disease. He was under the impression that, although the brain had become at last affected, that this was secondary, and arose from the strong sympathy between it and the stomach. As the present mode of treatment had been followed so energetically, without obtaining the wished for end, it was submitted whether it would not be better, under existing circumstances, to abandon this system entirely, and, giving up every remedy excepting such as were necessary to keep the bowels in good order, to depend upon the diet and regimen entirely for the cure: the diet and regimen to be employed for this purpose, to be similar to that used in Case 1st. After a long conversation, it was determined to continue the present method of treatment, excepting that the efforts to produce the full mercurial action should be continued with redoubled energy, and this was accordingly done to its fullest extent. I saw this case but a few times, for finding my views of it so radically different from the other gentlemen, I discontinued my visits, though I trust without any unfriendly feelings on either side. I understood that essentially the same course of treatment was pursued. The march of the disease, however, remained uninterrupted. The fits continued to recur more frequently towards the latter part of the time, the patient became affected with severe headaches, was occasionally delirious, and, shortly before death, strabismus and other strong indications of an organic affection of the

brain supervened, and he died on the 24th of Dec. 1818, about six weeks after I first saw him.

I have endeavoured, in the above statement, to mention, as clearly and as candidly as possible, every circumstance which appeared to me to throw light on this highly interesting case. All the facts stated were obtained from the bedside and the most direct and unquestionable authority, and noted at the time. It is not related with any intention of censuring the practice which was pursued. On the contrary, no man entertains a higher respect than myself for the medical attainments of all the gentlemen who had the management of it. No examination of the body, after death was made, yet there seems but little doubt that the head became, at least towards the termination of the case, affected. I have seen too much of the pride of opinion, and the bias given to the mind by preconceived notions in others, not to look with some degree of diffidence on my own impressions of the nature of this case. Yet it does appear to me, that there is great reason to suspect that the primary seat of the disease was the stomach, and not the brain.

I had selected several other cases which would illustrate more fully my views, but, finding the subject of this memoir insensibly expanding as I have advanced, until it has become more extended than the occasion requires, I must hasten to bring it to a conclusion.

The process by which the food is dissolved in the gastric juice has all the characters of a chemical solution. All the symptoms observed to take place in dyspepsia, seem to me to be primarily attributable to some imperfection in this process. Dyspepsia may be defined, a partial, or entire interruption in the first stage of digestion, the proximate cause of which is the diminished quantity, or vitiated quality of the gastric juice

We will now inquire whether the symptoms, which occur in this disease, can be satisfactorily explained by this principle. This will enable us to form more just opinions of the indications of cure. If animal and vegetable food be well masticated, and mixed with the saliva, when it arrives in the stomach, it will be in a semifluid form. If the food be in due quantity, and proper

quality, and if the gastric juice be in the quantity or quality usual in health, then, by the alternate contraction and relaxation of the stomach, successive laminæ of the contained food will be brought in contact with the internal coat of this organ, and will become saturated with the gastric juice, and, as it becomes dissolved, will pass in a more fluid form, through the pyloric orifice, into the duodenum. In this way a common meal is found to be dissolved in the course of 4 or 5 hours. But if the food should be excessive in quantity, imperfectly masticated, or consist of those substances on which the gastric juice has been observed to act less readily; or if any powerful impression were made on the general system, by agitation of mind, sudden change of temperature, a blow on the head, &c. &c. then the consequence would probably be that the function of the stomach would be disturbed, and its secretions less abundant in quantity and less efficient in quality. If this were the case only in a slight degree, then perhaps the time which it would take to dissolve, instead of being 4 would be 8 hours. This will account for all those slighter feelings of indisposition arising from slow digestion, which most persons occasionally feel, such as heartburn, slight pain in the stomach and bowels, acid eructations, headach, &c. Suppose that, either in consequence of the mode of life which has been already described, or from any other cause, the functions of the stomach were very seriously impaired, what would be the consequence of eating heartily of indigestible aliment while the body was in this state? We can describe some of the probable consequences, both from reason and experience. When food consisting of animal and vegetable substances is received into the stomach, it will be either acted upon by the gastric juice and run through the first stage of digestion, or it will obey the same chemical laws by which it is controlled in the same temperature out of the body. Under these circumstances then the stomach would be oppressed for many hours by its contents, which could not fail to cause various disagreeable sensations, either in the organ itself, or from sympathy, in some other. We might anticipate, what observation would confirm, that a person in this situation would be subject to all the intermediate degrees of suffering, from unpleasant sensation in the

stomach, and headach, to complete apoplexy. If the food did not become dissolved in the gastric juice, then, if it were vegetable, it would run into a state of fermentation, which would cause a production of acetic acid, and a disengagement of gas, of which circumstance the patient would be apprised by acid eructations and heartburn, &c. But animal food, when it is received into the stomach, is less apt to run through a chemical change. Even if the gastric juice should not be sufficient to dissolve it, yet it is possessed of such antiseptic qualities, that very small quantities of it would prevent putrefaction. Under these circumstances then, after various and severe sufferings, the stomach would either relieve itself, by rejecting its contents, or the food having passed into the alimentary canal in this crude state, it would cause flatulence, pain, constipation and colic, or diarrhœa, in which the food would pass off unchanged.

No man of observation can be long familiar with the phenomena of disease, without being deeply impressed with the extensive sympathies, and powerful influence exerted upon the rest of the system by the stomach; nor is it possible that he should be a very successful practitioner without laying a proper emphasis on this point. From the habit which some authors have of constantly adverting to the state of the heart and arteries, one would suppose that they were the only parts, the functions of which it was important to attend to in the management of disease. I am fully aware of the great value of the information to be derived from the pulse, in many morbid conditions of the body, and should be sorry to appear to underrate this highly useful index to disease; still I must acknowledge that I am under the impression that an undue consideration is attached to it, in the modern practice of physic, to the neglect of other important points. The stomach is almost the only organ indispensably necessary to animals. At least this would seem to be the case from the fact, that, although there are many animals which have no heart, arteries, or lungs, and others in which the existence of a brain and nervous system has not been demonstrated, yet we know of none which does not possess an organ which performs the function of a stomach. This is the only viscus which can be shewn in hydatids and polypi. No

powerful impression can be made on any part of the body, however remote, without the stomach manifesting its sympathy, nor can any serious derangement of this viscus take place without its effects being propagated to the remotest parts of the body.

When dyspepsia is symptomatic of other diseases, which is often the case, while we adopt the diet which the stomach the most easily manages, it will be obviously proper to devote our attention to the removal of the primary disease, before we can hope to restore the impaired functions of the stomach. Organic lesions of the liver, spleen, pancreas, &c. are almost always attended with Dyspepsia.*

But, when this exists as an idiopathic disease, there is no single article of the materia medica upon which we can place much reliance for the cure. Systematic writers lay great emphasis on the employment of mercurials, alkaline, bitter, and tonic medicines, to white oxide of bismuth, medicinal waters, &c. &c. The result of my own observations and experience is, that very little dependance can be placed upon them, but as temporary expedients for obviating particular symptoms. But the main pillar upon which we must rest, for the radical cure of dyspepsia, is a rigid adherence to a suitable diet and regimen.

There are few cases of idiopathic dyspepsia in which we shall not find our efforts very much facilitated by the early exhibition of an emetic. There is no class of remedies which appears to me to exert such prompt and controlling effects as emetics. This is remarkably the case in many diseases which make their attacks suddenly with alarming symptoms. In such cases we may often arrest the most painful and terrific symptoms in the midst of their career, and restore the system to the most perfect tranquillity, by the use of an emetic. The good effects of emetics do not appear solely attributable to the removal of the contents of the stomach; for it is often found that, when the quantity discharged is small, the relief obtained is very great. In addition to the good effects experienced from removing any irritating cause from this central point of the sympathies of the body, the mere effort of vomiting,

* I would again refer the reader to Mr. Abernethy's observations on this point from a conviction of their great value.

in such a depressed state of the vital powers, seems to produce powerful and salutary effects upon the nervous system. The appropriate use of this class of remedies will often supersede the prodigal employment of mercurials and the lancet, which, it must be acknowledged, are becoming too indiscriminate and empyrical in our country. After cleansing the stomach with an emetic, in the selection of a suitable diet for our patient, we should be governed by the following rules:

1st. It should consist of food most readily soluble in the gastric juice, and least liable to run through a chemical change.

2d. It should be exhibited in moderate quantities, and at regular intervals.

Our food consists of animal and vegetable substances, modified by different processes of cookery, which are known to render it more or less easily disposed of by the digestive organs. In the treatment of a Dyspeptic patient, we must prefer animal food, because it contains a greater quantity of nourishment in a given space, is most soluble in the gastric juice, and is less prone to undergo a chemical change. Of the different substances which come under the general denomination of animal food, the most desirable is the muscular part of those animals which are called game. Among domestic animals, the flesh of which is termed butcher's meat, those which are somewhat advanced in life, are more digestible than those which are young. Beef is more easily digested than veal, and mutton than lamb. Fresh pork at any age should be avoided, but the lean part, when converted into bacon, may be eaten as a condiment, though in small quantities. Fresh fish, alternately with meat, will at the same time gratify the taste and improve the health of the patient. Oysters are generally very palatable and proper, but they should be roasted sufficiently to destroy their vitality, and never eaten raw. In a word, game, poultry, especially the breast of a goose when divested of the skin and fat, the lean part of beef, and mutton made tender by keeping, and fresh fish, should constitute the principal part of the food of a dyspeptic patient.

But such are our habits in civilized society, and such the inclinations of persons suffering from this disease, that, except in ex-

treme cases, we shall find it not only unnecessary, but absolutely impossible, to confine our patients to a diet exclusively of animal food. We are so much in the habit of using bread during health that, in some form or other, it is indispensable. Indeed there is no vegetable food which can be eaten with more impunity than unfermented bread, e. g. ship bread. But bread which has undergone a partial fermentation, in which is included all the different kinds of soft bread, cake, &c. should be avoided. Of the different culinary plants, there is none which can be considered entirely innocent; onions well boiled perhaps approach nearest this character. But cabbage, turnips, potatoes, pastry, confectionary, nuts and fruits preserved in sugar, can seldom be eaten by a subject of this disease with impunity. Those plants which we are in the habit of eating uncooked, and which still retain their vitality, such as celery, radishes, cucumbers, &c. are particularly pernicious. This is no doubt attributable to the circumstance that the gastric juice will not act on a living animal or plant.

There is still a third kind of food which does not distinctly arrange itself under the head of either animal or vegetable substances; e. g. milk, eggs, butter, &c. In those diseases where it is desirable to nourish the body by an ample, yet not too stimulating diet, as phthisis pulmonalis, &c. milk and eggs are found extremely well adapted to this purpose. But milk is often found difficult of digestion by persons in health which is indicated by its causing dulness and inclination to sleep. Eggs and milk therefore, though not so objectionable as some other things, should not be permitted in this disease, excepting occasionally in small quantities when formed into a custard. Butter when uncooked and perfectly fresh is not an improper article of diet. But butter, or any fatty substance, when it has been exposed to the effects of fire, or kept so long that it has become rancid, are very pernicious, seldom failing to produce very disagreeable sensations.

As different modes of preparing food undoubtedly influence in a very material degree its solubility in the gastric juice, it will be proper to say something on this subject. By boiling several hours the more nutritious part becomes dissolved in the water,

and the meat is rendered dry and less palatable. The best method of cooking meat for a dyspeptic patient is roasting or broiling with a quick fire. In this way the nutritious parts of the meat are preserved, and the necessity of preparing artificial gravies is superseded, by the natural juices of the meat, which are easily digested and pleasant to the taste.

It is a commonly received opinion that decoctions of animal substances, such as broths, soups, &c. are more digestible than solid meat. This opinion probably arose from some supposed analogy between them and the chyme. From frequent and careful observation on this subject I am perfectly satisfied that that opinion is as unfounded, as the analogy is unphilosophical. Though a certain degree of softness in the texture of the food is necessary to facilitate digestion, yet, as has been remarked by Mr. J. Hunter in his "Observations on the Animal Economy," substances may be rendered too soft; for a fluid is difficult of digestion; and we may observe that nature, having given us very few fluids as articles of food, to render these few fitter for the action of the digestive organs, a coagulating principle is provided to give them some degree of solidity.

The success of M. Dupuytren* in treating cases of preternatural anus, the consequence of strangulated hernia, drew persons afflicted with this complaint from all parts of France to the Hotel Dieu, and furnished M. Lallemand with opportunities of making some interesting observations on digestion, and particularly on the action of the stomach on different species of aliments. Having met with a short account of these observations, since this essay was written, I cannot resist the present opportunity of laying before my readers some of the results.

"All the patients thus affected who came under the notice of M. Lallemand, had renounced the use of fruits, leguminous vegetables, and all aliments, the basis of which is fecula. They observed, that these aliments furnished them but little support, and appeased their appetite but for the instant. All without exception, had been led by experience to eat animal food only. It was observed that vegetables remained in the stomach for a much less

* London Med. and Phys. Journal, vol. 40.

time than animal substances—generally about half as long. In all of these patients, beans, peas, potatoes, &c. although they had undergone culinary preparation, and formed into a sort of pottage, escaped, almost without having suffered any change. Crude fruits passed in small, compact portions, without having experienced the smallest alteration. Green vegetables were almost immediately expelled presenting their original colour and general appearance. Bread was retained for a long time, as well as meat boiled, but not so long as when roasted. The chymous fluid formed by the latter, was more smooth and uniform than that of the former substance, and the articles of which it was composed could not be recognized."

The stomach, like the other organs, is capable of performing a certain amount of duty with ease to itself and benefit to the general system. Like the other organs too, if it be taxed beyond this point, it either acquits itself imperfectly, or entirely fails; and such unnatural exertion is followed by at least a temporary reduction of its vital powers. If in health then, it be proper to be cautious in the quantity of our food, this is manifest that it is of the greatest moment in a disease where the vital powers of this part are more especially prostrated. It is therefore a maxim, fully as worthy of attention as any which has yet been mentioned in the management of this disease, to allow no more food at any one time than the stomach can easily dispose of. When eating from a variety of dishes, though a person may take but little from each, yet they are very apt to overload their stomach, without being aware of it. We should therefore advise our patient, however rich or inviting the repast, to begin his meal by selecting one dish, and to take from that as much at first as he may deem sufficient, and afterwards, on no account, to gratify his appetite further.

"Animals are so constituted as to require alternate periods of action and relaxation, and the duration of these periods must be proportioned to its intensity." If the exertion be violent it cannot long continue, but if moderate it may continue longer, but must at last cease in order that the machine may be invigorated to that degree which constitutes health, by remaining in a state of repose.

This remark is true of the individual organs which compose the body, with the exception of that class which are called vital. In no one of them can the application be made with more emphasis than to the stomach, particularly when labouring under this disease. We should therefore take especial care, not only not to overtax its exertions by large quantities of food at any one time, but only to give it after such intervals as will allow the viscus sufficient time to recover its tone. As habit is known to exert considerable influence on the functions of the body, it is important that the food should be given at fixed periods. The commonly received doctrine of the necessity of giving food frequently to persons much reduced by this disease, is absolutely pernicious. It is not the quantity of food recieved into the stomach, but that which is assimilated, which invigorates the body.

It is important to avoid taking large quantities of any kind of fluid. In the selection of drink we should be governed by the same principles which regulate our choice of food. On this ground we should avoid fermented liquors. Pure water, brandy properly diluted, and old madeira wine, in small quantities, are the most suitable drinks. For a morning and evening beverage, strong coffee with but little milk or sugar is best. Its operation seems that of a pleasant tonic bitter.

With respect to regimen, time will only allow me to observe, that the circumstance most to be attended to is active exercise in the the open air, without much regard to weather. Riding on horseback, and travelling are particularly adapted to this complaint, and should be liberally followed. After eating, the patient should remain in a state of rest for at least an hour, to allow the process of digestion to become completely established, after which he may exercise moderately. But the most violent exercise should be taken a few hours before breakfast and dinner when the stomach is empty.

One of the most uniformly troublesome symptoms of dyspepsia is constipation. This is an evil which requires as much address and management to remove as any connected with the disease. Active cathartics are very apt to derange the functions of the stomach, reduce the vigour of the body, to leave the bowels in

a constipated state, and to induce that peculiar condition of these organs which renders them incapable of performing their functions without the assistance of artificial stimuli. It is therefore better to make use of small doses of medicine which will increase the peristaltic motion of the bowels as occasion may require. Small doses of ipecac. given in pills of two or three grains each, I have sometimes found to answer this indication very well, five grains of the pil. rhæi. composit. I have also found particularly useful for this purpose, and in those cases where the functions of the liver are disturbed, a grain of calomel in each pill I have found to promote their beneficial effects. They should be taken when necessary an hour or two before breakfast in the morning.

But there is no remedy which is so admirably suited to remove this as the agitation of the body by riding on horseback. This, with small quantities of ripe fruit eaten in the forenoon, regularity in going to stool, and the occasional use of a mild laxative or enema, if persevered in, will almost uniformly be found effectual.

Frequent dry friction of the body, and the warm bath, were strongly recommended by the ancient physicians. They have the happiest effect in restoring the healthy functions of the skin, and are certainly too much neglected in modern practice. They are both of the greatest moment in the treatment of dyspepsia. Of those remedies usually denominated tonics, and which seem to assist in restoring the healthy action of the stomach, I have found Griffith's myrrh mixture the best. Among the bitters, I prefer an infusion of quassia.

These remarks on diet and regimen, may appear to some persons to be drawn out to an unnecessary length. But though I would wish to avoid the charge of prolixity, yet I am desirous of stating my opinions in such a manner that they shall not be mistaken. I have little doubt that the doctrines, here advanced, will be acknowledged by some, and the principles on which they are founded familiar to all. They are presented to the profession under a deep conviction that they will be found practically correct. I have been induced to say more on this subject from an impression that dietetics are too little regard-

ed in the modern practice of medicine. The too general divisions of the subject into phlogistic and antiphlogistic, seem now to be thought all that is worthy of the attention of the practitioner.

ART. V. *Correspondence between* JOSEPH KLAPP, M. D. of Philadelphia, and J. B. SUTHERLAND, Esq. *concerning the cases of Malignant Fever, admitted into the Philadelphia Alms-house Infirmary, in the summer of* 1820.

PHILADELPHIA ALMS-HOUSE,
Manager's Room, September 19, 1820.

DEAR SIR,

As there are many reports in circulation highly prejudicial to the city of Philadelphia, from statements that have gone abroad relative to the cases of Fever that have occurred in the Sugar-house attached to this Institution, during the present season (a sample* of which I inclose you, taken from the Evening Post of

* The following is the publication referred to, which appeared in the New-York Evening Post of the 15th of September.

On the 9th instant we published five queries, addressed to the President of the Board of Managers in Philadelphia, with the design of drawing from him certain facts interesting to the citizens of that place as well as this, and with the further design of exposing a course of management in suppressing the truth as to the existence of yellow fever there, not, in our opinion, quite reconcilable with fairness and integrity, nor very honourable to the majority of those concerned in it. The following answer, received by this morning's mail, reflects the highest credit on the bold and independent writer.

Philadelphia, Sept. 14.

To the Editor of the Evening Post:

Sir,—An article containing five questions appears in a paper of this city, stated to be extracted from the Evening Post. The paper of your's in which that article appeared, has not reached us, as well as many others. The questions are these:

"*Queries.*—We dont like to be troublesome to our neighbours, but really we cannot resist the inclination we feel to ask a few questions of somebody connected in someway with the Philadelphia Board of Health, or Board of Managers, or of any other person competent to answer.

"1. Is there such an establishment as the hospital of the alms-house in Philadelphia?

New York). You will confer a favour by giving me, as the visiting manager of the house for this week, a detailed account of all the cases that have come under your notice in the "Sugar-house" as Prescribing Physician to the Alms-house during the

"2. Was there not more than an average of 10 cases a week of fever, in that hospital, who actually died of black vomit, as early as last June? Or how many were there?

"3. Have these cases ever been made known to the public through any official report?

"4. Is there such an officer as President of such Board of Managers, and will he answer these questions?

"5. Can any body there or here, tell me how it is that the city of Philadelphia should originate yellow fever the present season, while Baltimore and New-York remain quite exempt from that disease?"

Answer 1. A house, formerly a sugar manufactory, spacious and roomy, has been employed as an hospital to the Alms-house. It was originally intended as a sort of lazaretto for the reception of sick paupers, and to prevent the introduction of infectious diseases into the body of the house. The law and regulations of the house do not authorise the admission of persons afflicted with malignant disease: I presume, however, there is a Board of Health.

Answer 2. I cannot at this moment ascertain the average, but on the 13th and 19th of May, there were deaths in that hospital of malignant fever—there have been at different times since from 10 to 30 sick at the same time—black vomit, and all the usual characteristics of malignant fever. The deaths have been numerous, and it has not been confined to black people, though they have been most numerous, for obvious reasons. The nurses have taken it—Mrs. Ballard, an English woman, a nurse, took it and died of it.

Answer 3. The health of the city being entrusted to a special Board, the Managers of the Alms-house did not consider it their duty to publish any thing on the subject. They applied to the Board of Health, and solicited permission to send their sick to the city hospital—the Board of Health did not even send an answer. The Board of Managers accordingly represented the case to the Board of Guardians, (44 in number,) who appointed a committee two successive weeks, who were equally unsuccessful—the hospital was refused to be opened. I do not think it necessary to state the reasons which were assigned for the refusal. The Board of Managers, as bound by law, resolved not to admit any case, without a certificate from a physician that the applicant was free from infectious disease; and none have been admitted since. The number of sick and the number of deaths have been accordingly reduced. There were 63 in the hospital when the resolution was past—there are only 20 now; but of these were, on Monday last, white men 5, white women 3, total 8; black men 6, black women 6, total 12—whole number 20, of whom eight were in a dangerous state.

Answer 4. There is a president, and he it is who answers these questions.

existence of malignant fever in this place. I am particularly anxious to know whether any cases of "Malignant Fever" have come under your notice in the Sugar-house.

Yours respectfully,

JOEL B. SUTHERLAND.

JOSEPH KLAPP, M. D.

Prescribing Physician to the Philadelphia Alms-house.

The fifth question he cannot answer—for when the doctors disagree, who can decide?

The paper here which publishes your questions makes some mistakes: he says *all* the cases are blacks. You will see by the above that this is an error. The majority no doubt were blacks: but there were many whites. Mrs. Ballard, who solicited to be employed as a nurse, caught the disease and died with black vomit: She was an English woman; sedate, sober, and correct in her habits and demeanour. Many other nurses have died in this hospital.*

As to the physicians paying a fine, the fact proves that there are points which legislation cannot approach. Who can fine and who can define the certainty of an opinion? A physician knows a hawk from a hand-saw, that is when the wind's S.; but if his theory be a point to windward or leeward, where will you fix him? He will tell you that the disease is never alike in two seasons, nor in two persons in the same season, and that bleeding, which was salvation in one year, is damnation in another, and salivation which saved every thing once, destroys every thing now; that turpentine in 1820, is as potent as lime in 1798. Such is the *certainty* of the system that the Board of Health intimated to a committee of the guardians of the poor, that if all the physicians of the city were to certify, that a person had the malignant fever, they would not regard it unless sheir own physician of the port should say it was yellow fever. The law is, in effect, a nullity, as it at present stands, and subject to such caprices, cannot inspire either confidence or hope: nor does it afford the Board of Health itself the power to act with decision and vigour; nor is the local influence of money and traffic a slight difficulty to any Board of Health.

If any further explanation should be desired, and they are in my power to give, I shall not hesitate to answer, though I have considered my present station as a public agent required of me to refer what concerned the public to the body whose peculiar duty it was to take care of the public health.

The President of the Managers of the Philadelphia Alms-House.

* *Nota bene*—School of domestic origin and non-contagionists, what say ye to the fact here stated, and which you have so often denied?—*Ed. E. Post.*

Philadelphia, Sept. 27th, 1820.

MY DEAR SIR,

In reply to your favour received on the 19th inst. I beg leave to tender you with the following statement. As one of the physicians to the Alms-house Infirmary, I entered on my present tour of duty in the beginning of July, and the subjoined account, addressed to me, by Mr. Samuel Jones, one of the resident pupils of the institution, will accurately exhibit to you, both the condition of the fever hospital at that time, and since to the 22d of the present month. The number of patients (says Mr. Jones) in the sugar-house, or fever hospital, when you took charge of it July 1st, was about 20. Many were admitted in a dying state, and expired in a few hours. Such was the ingress at this time, that notwithstanding there were deaths of 3 to 6 per week, and discharges of many more cured, the number in the house July 31st, was 49. On the 7th of August there were 63; during the succeeding week half of the whole number was discharged cured, and five died. During the week previous to the 7th of August, five also died. August 15th, 39; August 21st, 43: during the week ending to-day, there have been 6 deaths. September 4th, 33: 7 white, 26 black. In the last week there have been 6 deaths. September 18th, 14: 1 white, 13 black. September 23d, 13: one died last night. The two worst cases now in the sugar-house are tedious cases of relapse, but are recovering, all others are fast convalescing. By the end of another week the closing of the sugar-house may be reasonably anticipated. I believe the whole number seen by you in that house exceeded 200, of these as many as 45 died.

I shall for the present take leave of Mr. Jones's narrative, to offer a few remarks on the origin, and general character of the complaints with which our patients were affected. The sick paupers in question, have been chiefly derived from Atkinson's court, Pine alley, Miles court, and from other equally filthy, and sickly places, both in the city, and liberties. The particular forms of disease which they laboured under, appeared to be the low remitting fever, and the different grades of typhus. These complaints are such as physicians are accustomed to meet with,

at this period of the year, but their particular prevalence this season among the blacks must be attributed to a general want of employment, to vicious as well as the indolent habits, which many of the lower class of these people are known to be addicted to. And further, their scanty, and often unwholesome diet, with a habitual disregard of cleanliness, both personal and in their dwellings, have doubtless contributed much to disseminate the disease. Many of them pass through the first stages of their disease, before their situation becomes known to the Guardians of the poor, and consequently (as Mr. Jones has already asserted) are put down at the door of our hospital either in a state of actual dissolution, or they have been so far exhausted as to preclude all hope of deriving benefit from medical applications. Under such circumstances, the mortality of the disease should cease to be a matter of surprise. The typhus disease, was the one most prevalent. It was usually introduced by a chill, and considerable subsequent excitement of the circulation, by pain in the head and vertigo, much soreness, and tension of the epigastrium, a white tongue, and a prostration of the muscular system. Much general debility usually appeared between the 3d and 5th days, and the disease in many instances came to a termination between the 7th and 10th days. Relapses were frequent in the first stage of the disease. Local bleeding, sudorifics, and blisters, were found most serviceable; when further advanced, stimulants, cordials, and mercurials, did most good. The principal determinations were to the head and stomach. In the latter respect there was a difference from ordinary typhus, and from its being a symptom common to both it, and yellow fever, some were at first disposed to believe them the same. This opinion, however, obtained to a very limited extent, and I believe in the latter part of the season, when all had an opportunity of fairly contrasting them, the opinion of their identity was relinquished by every one. A circumstance evincing the diversity of their character, is the fact of their prevailing in very different districts of our city. I do not recollect to have heard of a single well marked case of yellow fever, originating in the dirty alleys, remote from the river Delaware, though abounding throughout the summer with cases of typhus,

intermittents, and remittents. My public and private practice afforded me many opportunities, of instituting a comparison between the symptoms of the two epidemics, and which has enabled me to detect the following differences between them. First, in the typhus of the blacks, in the very first stage the epigastric region evidences great sensibility to the touch, whereas in the malignant yellow fever this sensibility does not usually occur until about the 3d day. The particular seat of this morbid sensibility is different in the two diseases; in the former it is more external, while in the latter it is chiefly internal. 2dly, in the typhus the most active as well as the most unpleasant remedies are generally retained with ease on the stomach; not so in the yellow fever. 3dly, in the different stages of the typhus the tongue undergoes the several changes characteristic of that disease, but in yellow fever it presents a different appearance; in no case of the latter disease, have I met with the hard dry and glossy tongue, the usual attendant on the former. 4thly, hæmorrhages in the second and last stages of the typhus are uncommon, but very common in yellow fever. 5thly, in the typhus there is an early prostration of the muscular powers, not so in yellow fever. 6thly, the yellow, or icteric eye, marks the first stage of the typhus, but in yellow fever it rarely occurs before the 3d day. 7thly, black vomit very common in the second and last stages of yellow fever, and very uncommon in the typhus of the coloured people. 8thly, the convalescent state of yellow fever is generally short, and as generally tedious in the typhus.

Enough I trust has been said to give you an idea of the principal diseases our patients have laboured under; in the next place I will endeavour to answer your question, whether any cases of malignant disease have occurred in the sugar-house, since the appearance of yellow fever in our city. I answer, there have been a few cases. The epithet *malignant* has an extensive application. Any disease may become malignant under adequate circumstances of aggravation; accordingly we find the most approved authors enumerating malignant small-pox—malignant dysentery—scarlatina, &c. &c. It must therefore be kept in mind, that every case of malignant disease is not to be regarded as

yellow fever. It is not a very uncommon occurrence for intermittents and remittents under certain circumstances to become malignant in their edvanced stages. At this moment I can recollect to have met with several cases of those fevers in my practice which have been attended with the black vomit. During the present summer I have seen two instances, one was a well marked intermittent in which black vomit occured twenty-four hours before death, and in the other, a bilious remittent, that symptom took place so late as the 17th day, and the patient recovered. To denominate every disease, yellow fever which happens to be attended with coffee-ground discharges from the stomach, would indeed create great confusion in the syllabus. For example it has occured in cramp of the stomach, in pleurisy of the winter season, in cases of gastritis induced by drinking cold water when the body has been much heated, in the same affection from the kick of a horse on the abdomen, in dropsy, in consumption, &c.

I therefore infer that it would be as palpably erroneous to call a bilious fever, or a typhus, malignant yellow fever, because in some instances those affections have been attended with black vomit, as it would be to attach that dreaded appellation to a colic, or a pleurisy, because they also have been known to be attended with the same symptom. Having now expressed the distinction, so far at least as may be sufficient for my present purpose, between the aggravated state of the ordinary diseases of the season, and the yellow fever, relative to which alone it is conceived the Board of Health possess a legal jurisdiction, I will further state in reply to your last inquiry, that though we have had some cases of remitting fever, and typhus, both in the sugar-house, and in the main building, which in their worst states have taken on appearances of malignancy, *yet I am not sensible of a single instance of true malignant yellow fever having occurred in either of those places from the beginning of July, up to the present period.* Out of more than 200 examples of the spring and summer fevers, it is with me rather a subject of surprise, that we should not have met with a greater number of cases running into malignancy.

In order that you may be supplied with full, and the most un-

reserved information of the state of the sick in the sugar-house, I will extract from Mr. Jones's communication addressed to me, a description of the worst and most suspicious cases which have been noticed in the fever hospital during this season. " There have been (says that ingenious, and most indefatigable student) five cases of something like black vomit; two of these were admitted in the first week of August, one in a dying state, and expired in a few hours after admission. She appeared to have been longer sick, though I was informed by the person who brought her, she had been sick but three days. She, however, was not able to give any satisfactory account of the case; she was cold and insensible, and unable to swallow any thing, but threw up at different times small quantities of a dark matter resembling coffee grounds. The other was yet able to speak for herself, said she had been ill for five days, was taken with bilious vomiting, headach, chills, pain in the back, &c. was at this time extremely low, pulse small and compressible, the whole surface cold, tongue foul, eyes suffused, some appearance of blood from the nose, tension and tenderness of the abdomen; she soon sunk into a low muttering delirium, and an hour or two before she expired, some feeble efforts were made to vomit, and she brought up a quantity of matter, indeed hard to be described; at first sight, though much darker, and more consistent, yet not very unlike the grounds of chocolate. This discharge when attentively examined appeared to be composed of a mucous secretion of blood and the common contents of the stomach, intimately blended.

The week following, on the morning of your attendance, we were visited by Drs. Jackson and Burden of the Board of Health, and also Dr. Knight the Port physician, who then heard all these circumstances, approved of our opinion and conduct. They examined every disease, inquired into the history of every case, and were all satisfied, we had not then nor at any time previous, disease, that should come under the notice of the Board of Health. The gentlemen of the Board of Health have since visited us; many of the most respectable physicians of the city have seen our patients, and it has never been our opinion, or surmised by any one, in whose opinions we have confidence, that there was

any thing demanding the notice or interposition of the Board of Health.

In the week preceding the visit mentioned above, a white woman was admitted apparently in a dying state. The history I could obtain of her case was equally imperfect with those above mentioned. I learned, however, that she had been for years the prey of the venereal disease, that in consequence her frame was emaciated and running into decay. I was further informed that she had suffered about three weeks from the fever, the immediate cause of her being brought to this house. At this time her extremities were œdematous and cold, pulse at the wrist imperceptible, tongue black, and thickly incrusted. Her fate was pictured in every feature. This kind of existence was protracted four days, by the administration of cordials, the external application of heat, and other stimulants. In the mean time by feeble efforts to vomit, her mouth was frequently loaded with a matter resembling in its consistence that described above, but much darker, and very offensive to the smell. The next case was that of Thomas Jones, a mulatto, aged about 40 years; his disease appeared to have been a case of gastritis, induced as he said by drinking cold water at a time when his system was highly excited by exercise. He had much pain and tenderness of the stomach, and always shrunk from the touch. He had when admitted a very excited pulse, parched tongue, and great heat of skin. Bloodletting was repeated in small quantities, a perpetual blister was kept over the seat of the disease, and managed in other respects according to circumstances. He remained some days a doubtful case, was seen occasionally to vomit a matter which appeared to me the product of inflammation, complicated with the contents of the stomach, not unlike what I have been accustomed to see as noticed by you in mania a potu, till finally it appeared to me the more genuine coffee-ground black vomit. In the mean while so exquisite was the sensibility of his stomach, that he suffered much from the least change of position. At this time every symptom declared his danger, he was notwithstanding recovered. The last case was that of Sarah White; the sugar-house being then under the care of Mr. Wilson, I am less familiar with it. I

think she had been ill near three weeks, when she came in was bled repeatedly, and convalesced, was able to walk up stairs, where she immediately relapsed, and in one week died. The black vomit in her case was seen only, and described by the nurse, an honest woman; she survived about 12 hours after its appearance. After death the stomach was examined, and did not exhibit any appearance of inflammation; the matter contained by it, but little resembled black vomit.

More than 20 nurses selected from the paupers of the house were employed in the hospital, many of these took sick; this was in part attributed to the foulness of the air of the wards they had charge of, to loss of sleep, fatigue, and intemperance.

The circumstances of Mrs. Ballard's case are more affecting. She was once acquainted with better fortune, but circumstances not in human control, obliged her to take refuge in this house. With the view to become useful, she offered her services, and was for some weeks employed as principal nurse. She was about 42 years of age, of the most approved habits, and endowed with an active mind. She felt deeply the importance of her trust. Most amiable, and meritorious woman, possessed of too great susceptibility, and sympathy of heart to close an eye, to take rest, or even the food that was necessary, whilst she was able to carry relief to some sufferer, and in the administration of medicine could only confide in the honesty of her own hand. Though the necessity of self-preservation was pointed out to her, she daringly persevered for weeks, until August 20th, when, as might be anticipated, she was more powerfully admonished by a severe headach, chill, burning fever, and bilious vomiting. These symptoms were relieved for a time by rest, bleeding, and an emetic. Mercurial, and other cathartics, pediluvium, sinapisms, diaphoretics, blisters, &c. were resorted to, but the force of the cause was great, and the distemper was comparatively unyielding. Even on the 5th day, the fever resisted every effort. When the circulation became languid, the head much pained, expression wild, and anxious, her features shrunk, the extremities lost their temperature, she derived some relief from cupping glasses to her forehead; at this time her head was shaved and blistered, external stimulants, and

frictions were resorted to, which seemed all to affect her favourably. But her stomach was too irritable to be appeased, or to retain cordials, now so strongly indicated. Finally, the facies hippocratica, with low delirium occurred, and the energies of life held out to the eighth day. *Though she vomited much early in the disease, and sometimes later, no one saw, or at any time suspected the appearance of black vomit.*"

In relation to the question of the communicable nature of the disease from one person to another, *I hesitate not to affirm that no fair instance of its possessing that quality, has come under my notice.* On the contrary, we have been furnished with proofs of its not being contagious. Some of the nurses, including Mrs. Ballard, on their contracting the disease in the sugar-house, were received for better air and accommodation, into different wards of the Infirmary, and though they were surrounded by many paupers, in no instance was the disease communicated. Had it been contagious, or catching from one to another, I ask why did it not manifest that character within the walls of the Alms-house?

With much regard, I remain

Your friend, and humble servant,

JOSEPH KLAPP.

To J. B. SUTHERLAND, Esq.
One of the Visiting Managers of the Philadelphia Alms-house.

ART. VI. *On the employment of the Electric Fluid in obstructions of the Biliary Ducts, and in various diseases of the Skin.* By RICHARD W. HALL, M. D. Professor of Midwifery in the University of Maryland.

THE exhibition of the electric fluid, with a therapeutic intention, is still adopted only in a limited degree. I propose at present, to invite the attention of the profession to its success, in obstructions of the *biliary ducts*, and in *various diseases* of the skin.

An account of the success of electric shocks, from a jar of moderate size, in promoting the passage of a gall-stone through the

biliary ducts, was published in the Transactions of the College of Physicians of Philadelphia, about the year 1795, by my father Dr. Jacob Hall. The experiment was made in his own case. The obstruction to the discharge of the biliary secretion was attended by a jaundice, absence of bile in the fæces, and by incessant and acute pain in the region of the liver. After having recourse to various articles of the emetic, cathartic, and anodyne characters, without benefit, he was induced from his intimate knowledge of electricity, and its reputed activity in several diseases, to direct, that successive discharges from a jar of moderate size, should be passed through the right hypochondriac region from various points. The removal of the obstruction was the immediate consequence, as was evinced by a profuse and sudden alvine discharge of bilious matter; the pain of the liver ceased forthwith, and the yellowness gradually declined. This case then stood without a precedent, on the records of medicine.

In treating of obstructions of the liver, Dr. Darwin recommends a similar practice, which he *expressly states* was derived from the *above source.** He also details several cases in which this practice was successfully adopted.† "By my directions (he observes) a gentleman who had a jaundice six weeks without pain, sickness or fever, had taken emetics, cathartics, mercury, &c. to no purpose. On a supposition that the obstruction of the bile was owing to a torpid action of the biliary ducts, ten smart electric shocks were directed through the liver; on the same day the fæces demonstrated the removal of the obstruction, and the passage of the bile through its natural channel. The shocks were continued a few days more and his skin gradually became clear."

Since the period of the report above alluded to, it gives me pleasure to be able to add to the testimony already adduced in support of the utility of electricity, the results of many cases of biliary obstruction, attended with jaundice, both with and without pain, in which the favourable effects of this practice were prompt and complete.

For several years past I have also made a cautious application

* See Zoonomia, vol. 2, quarto, London, 1796.

† Zoonomia, vol. 1 and 2, 9th, London.

of the electric fluid in the modification of slight shocks or strong sparks in the jaundice of pregnant women; more particularly when uncombined with inflammatory symptoms either local or general. The obstetric class in the University of Maryland has been made acquainted with these facts and results; and I have further had the satisfaction to learn that the practice has succeeded to the wishes of several of our alumni.

A case of chronic icterus of nine months continuance in the mother of a family (aged 35) was submitted to my professional care last summer, in the month of June. Occasional pain, intense yellowness or a bombycinious colour of the whole body obtained, with emaciation, nausea, constipation, tenderness of the liver, and white or ash-coloured alvine evacuations; added to these, a morbid expansion of the pupils of the eyes, and limited and imperfect vision, even of large objects. Mercury, cathartics, &c. had been used previous to my first visit; but, such was now the irritable condition of the stomach and bowels, that mercury could be rendered tolerable under no form as an internal prescription. About 20 electric shocks were transmitted through the region of the liver once or twice a week. A few days after their exhibition the fæces attested the mixture of the bile, and her general improvement has been uniform, though gradual. Her vision and digestion are much improved, and the skin has lost much of its former yellowness. Small portions of mercurials can now be borne by the stomach, and without doubt aid much in her recovery.

But the beneficial results of the electric fluid have not been confined in our hands to obstructions of the biliary ducts, &c. In the year 1803, by my father's directions, this fluid was applied in the form of sparks to that diseased condition of the skin which Dr. Willan denominates *Psoriasis Palmaria.* In the first case which was of three years standing, one general application of the sparks to the surface of the hands (the parts affected) subdued the disease; the parts thus operated on became swollen and painful, and the patient complained of formication. An emollient cataplasm was directed, and the diseased skin in a few days was totally removed, and the parts beneath left smooth, pli-

ant and healthy. The second case was of seven years continuance; and the hands in this, though very stiff and scaly, were less irritable. By one operation of the electric sparks, the disease was removed; both these cases were females of middle age: their results (which have been since confirmed by the events of a great number of cases nearly allied to these) induced me to apply the electric fluid in a distressing case of the "*impetigo figurata*" of Dr. Willan.

In the spring of 1805, a man aged about 35, applied to me, with this eruption extending itself over the superior portion of the scalp, and invading the nose, and ears, and also his eye-lids to such a degree as to cause a privation of sight for several successive weeks; the disease occasionally remitted in its violence, and again confined him to his room by its exacerbations. When I first saw him the disease was of eighteen months duration; and he assured me that no medical skill had been left unemployed which this city could offer; under these circumstances I determined to trust exclusively to the electric sparks, which were powerfully drawn from the parts affected by means of a machine of ordinary size; they were repeated twice a week; the third application completed the cure. In describing the sensation excited in the part immediately after the application of the fluid, he observed, that it was similar to that excited by a swarm of bees moving over the head. At the expiration of eight years from this time he had no return of the disease. I have been more minute in this detail, as it is the only case of the Impetigo Figurata, which has fallen under my notice.

In *many* cases of the "*psoriasis diffusa*" the same remedy has succeeded. Indeed I have seen none which have resisted the repeated application of the electric sparks, and within a few weeks, a case, of more than usual obstinacy, in the face and eyelids of a young lady has been totally subdued by this means.*

In the course of my own practice I have also succeeded in overcoming, by the above means, many of the most unpleasant cases of "*porrigo favosa*," or scald head. But the remedy, with

* Vid. 13 and 14 plates of Alibert, termed "Dartre Squammeuse humide et orbiculaire." Liv. iii.

me, has not been so *uniformly* successful as in the preceding diseases, though I have received many communications of its success in this disease from my professional friends in the country; as also in several species of herpes, as they describe them, which I suppose to be the *Psoriasis Palmaria* and *Diffusa.*

I have endeavoured to extend the benefits of this practice to the "Porrigo *Scutulata*" and *Decalvans*, but without success; and I have now under this treatment a case of *Acne Indurata.*

ART. VII. *Case of Popliteal Aneurism, with observations upon Aneurism in general.* By HORATIO GATES JAMESON, M. D. of Baltimore.

JAMES BROWN, a muscular and well formed man, aged about 35 years, was admitted into the Baltimore hospital, on the 13th of June, 1820, on account of a painful swelling of the leg. He stated that he had been engaged some weeks previous, at Amsterdam, in unlading a vessel of tobacco; and in doing this, he was in the habit of lying on his back and rolling the hogsheads by applying his feet. This necessarily required great exertion of the muscles having their origin or insertion about the ham. But, although he gave us this information, he repeatedly declared that he had not been sensible of any injury having been done to the ham; that he had not been aware of any tumour, pulsation, or any impediment in walking, till some days after the unlading of the vessel.

Shortly after getting out of port his foot became painful, and seemed to be fretted across the instep by his boot. On drawing the boot, he observed a small abrasion of the skin on the instep, and that his foot was considerably swelled. The foot soon became extremely painful, and tumid; and at an early period of this affection of the foot, a severe pain was felt up the side of the leg, (along the course of the tibial artery.) The whole limb became speedily painful and œdematous.

This state of things continued during his passage homewards, and he suffered the most agonizing pain during thirty days, with-

out medical advice. Being greatly distressed at the time of his admission, and none of the physicians to the house present, at the moment, the steward directed a poultice of bran and vinegar over the whole swelling. This afforded so much relief, that Dr. Dorsey, the house physician, thought proper to continue it.

When I first saw him, the poultice was losing its effects, and fomentations of bitter herbs were directed. This also afforded considerable relief, for a day or two, and then, lost its influence. I found the tumefaction extended over the whole foot and leg; and terminating just above the ham. There was a considerable tumour in the ham of a stony hardness; or perhaps, it would be more correct to say, that this enormous swelling of the limb terminated just above the ham, and the only thing remarkable here, was the greater hardness of this part of the swelling. This more dense part presented itself in three prominences, not very remarkable, but, distinguishable by a close examination. The most remarkable of these, was the one on the inner side of the knee, the next in size was betwixt the tendons of the hamstring muscles, and the least prominence on the outside of the biceps muscle.

Aneurism was suspected, but, the patient had not been sensible of any injury having been done to the ham, nor of the existence of a tumour, or of any pain or pulsation in the part; on the contrary he declared positively, that the pain and swelling had commenced in the foot, and progressed upwards. Two or three days after his admission, *his right foot* also began to swell, became speedily painful and greatly œdematous, and the patient insisted, that the disease of the left limb had began precisely as that of the right. The skin of the left leg was still of its natural colour, but the veins were somewhat turgid; and the limb was above the healthy temperature.

The fomentations which have been mentioned lost their effects entirely, and the patient was reduced to a most pitiable condition, being unabled, for many days and nights, to get to bed or enjoy any sleep; the pain being, as he said, tenfold greater when he laid down. Mild anodynes had been used from the beginning, but they were now increased in strength, and frequently repeated.

Purgatives were directed to be given occasionally. A large blister applied over the whole limb acted well and afforded some alleviation of his sufferings for two or three days.

At the end of the first week, after his admission, the poor man was so worn down by the total want of sleep, that I felt confident nothing but amputation of the leg could save his life. A consultation was called of several highly respectable medical men, but it so happened that, on that day, there had been some little alleviation of the pain, and the patient being no doubt greatly alarmed, at the presence of so many medical gentlemen, declared himself somewhat better.

The pain was now greater than it had ever been before, and the patient could neither go to bed, nor sleep in a recumbent posture; the whole limb was greatly heated, and œdematous, the skin peeling off from the foot, which last could only be viewed as an effect of incipient gangrene. My opinion, now was, that there could scarcely be a doubt of the rupture of some deep-seated vessel; that it was not easy to conceive how any thing but the rupture of a nerve, or the extravasation of blood or lymph, in such quantity as to act as an extraneous body, could occasion such extreme pain; that, as the limb could only be kept in a depending position, gangrene would certainly progress, that the cause of the pain, be it what it might, was of so serious a nature as to justify amputation; because, the total want of rest, which existed, would either prostrate the general system to an irrecoverable degree, or that mortification of the foot would certainly take place; and as the falling off of the skin did not yet actually amount to gangrene, that, by removing the limb, and thus relieving the general system, we might expect some safety for our patient.

But the gentlemen in consultation not having seen so much of the case as I had, and finding the man somewhat better; and learning also another strange fact, which I have already noticed, that the other foot had swelled after his admission; and that he had repeatedly stated, that the disease of the limb first affected, commenced precisely as that of the last: they were, manifestly, surprised that I should have viewed the case in so serious a light as to require amputation. And, indeed, finding no seconding voice,

and being more particularly puzzled at the circumstance of the other foot swelling, and becoming painful as the first; I felt so much mortified for the moment, that I did not go into any precise exposition of my opinion, which had been delivered to the young gentlemen, resident students, the day before. The gentlemen present had different opinions: it was supposed to be a case of phlegmasia dolens; common phlegmonous inflammation, which would lead to extensive suppuration; that matter was now forming about the ankle; phlegmonoid erysipelas was also mentioned. But, unfortunately, this case, like some perplexing cases of hemorrhagy from a wounded artery, seemed to conceal itself at this time, and the patient, who by the by, was extremely unintelligible, seemed now unable either to express his sufferings, or to give a correct narrative of his case.

The day previous to the consultation, I had witnessed the anguish of the patient, had observed his hollow eyes, a hot burning skin, a feeble circulation, his heart-rending moanings, his head hanging, as it were, upon his fatigued neck, stupified from opium, which could neither afford sleep, nor mitigate his sufferings; and, in addition to all these accumulated sufferings, the skin was peeling off from his foot. Under these circumstances, I thought myself justifiable in calling a consultation. But when I heard no supporting voice, and the appearances on that day were so much against me, I became so distrustful of my own opinion, that, after merely mentioning my conviction of some extravasation, I readily gave way to the general opposing voice.

This, I fear, was an unlucky day for our patient, but still, I trust, those who are best acquainted with the difficulties and doubt which sometimes obscure cases, and who, take into view the circumstances detailed in the history of this case, will most readily exonerate the gentlemen on this consultation, as well as myself, from any particular want of discrimination: for as none of the most characteristic symptoms were present, or had ever been observed by our patient, how could we discover aneurism? We had assuredly nothing but vague evidence. As there was a low state of action of the sanguiferous vessels, and the skin of the foot presenting strong appearances of gangrene, Dr. Mackenzie pro-

posed the free use of the cinchona, and anodynes pro re nata. This was adopted.

The pain continued with aggravated horrors, notwithstanding large anodynes were frequently repeated, and sphacelation of the foot and about the ankle soon presented itself. The bark was now ordered in the largest quantities the patient could be persuaded to take it. He took very large quantities with much resolution, and the gragrene became stationary for two or three days. About this time severe pains occurred in the back, and cramps extended often up along the thigh; the whole constituting a scene of unutterable anguish.

On the twenty-ninth it was determined in consultation, that the amputation of the limb was the only resource left us, towards saving the patient's life; and that as the gangrene had become stationary, it would be no material departure from the good old rule, of delaying the operation till gangrene was arrested. The pain continuing equally severe, and the patient becoming rapidly weaker, it was thought that such extreme pain must soon exhaust the man, unless the general system was relieved from the annoyance of the diseased limb. I had never abandoned the idea of some extraneous body being lodged in the limb, from a ruptured vessel. And I find that I mentioned to my friend Dr. Bain, of this city, the day after the first consultation, "that, I believed it would turn out to be a case of popliteal aneurism, or that some deep-seated vessel had been ruptured."

At 11 o'clock on the 30th of June, the 17th day after his admission into the hospital, the patient was carried to the operation room. He was very week, but apparently as free from fear as could be expected; and I had now to engage in an operation almost hopeless; but as it seemed to afford some hope, and as speedy death seemed otherwise nearly at hand, I considered it my duty to operate. He bore the operation well, and repeatedly declared, that the pains of the operation were much less severe than those he had long suffered. He seemed so revive, his pulse became fuller and more tranquil, his countenance brightened; and he retired to his bed so well, that I had strong hopes of his recovery. Two hours afterwards I saw him still more revived,

free from pain, his pulse equable and tolerably full. But shortly after this he became sick at stomach, had chills, complained of loss of vision, faintness, &c. and after a few hours suffering he died, the same night.

Dissection of the Leg.—My attention had, at all times, been particularly turned towards the ham. And having gotten hold of the leg, I dissected off the integuments from the greatest prominence, already mentioned, as being on the inner side of the knee. The integuments being removed, the muscles having a livid appearance came into view; but it may be remarked that the skin had its natural colour. A free incision was made through the muscles, when large clots of blood made their way through the incision, with some force, owing to the great tension of the muscles. At least two pounds of coagulated blood, having a slightly putrid smell, were taken out of the sac or cavity, formed in the ham. The whole cavity, at a first view, presented a horrible spectacle.

The popliteal artery was burst out into a ragged form, its upper side, or that towards the patella, had some slight attachments, but the lower side was clearly divested of its cellular envelope. The artery was spread out somewhat like a leaf, being about one and a fourth inch in its longest diameter, which ran with the course of the artery, and about one inch in its transverse diameter; the edges of this leaf-like and lacerated artery, were red, having a livid shade; these lacerated edges had some resemblance to a cock's comb, and were about half a line in thickness; (an evidence surely in this case, that the coats of the artery had not grown thinner, but on the contrary, much thicker). The artery lay at the bottom of the sac, speaking of it as examined with the patella turned downwards, and was insulated a small distance, being surrounded by the coagula. The artery on one side was divested of its cellulur sheath in part, quite down below the bifurcation of it. Above the cavity, the artery seemed to be healthy and covered with its proper membranes, which was ascertained by splitting the vessel from above downwards into the cavity. But on cutting down to the tibial artery, along the tibialis anticus, and attempting to pass a small probe upwards, it readily run

through the side of the artery; strong evidence assuredly of a diseased state of the coats.

The coagulum was crammed under the gastrocnemii muscles, forcing upwards so as to put these muscles, and the hamstring tendons, greatly on the stretch. A projection was formed outside of each hamstring; and a third between these tendons: then the coagulum extended downwards, as far as the termination of the fleshy fibres of the gastrocnemii muscles, and forming a great mass of coagulum having regularly the shape of a wedge.

The muscles, membranes, aponeurosis, and all the parts forming the walls of this cavity, for there was no regular sac, had a frayed appearance. I think if we were to imagine, that a body having short strong cutting teeth, as a card of wire, for instance, had been freely turned round in this cavity we should have a tolerable notion of the frayed, lacerated, or torn appearance of its walls. About four inches of the upper part of the bone was stripped to its periosteum, and this had the same frayed appearance as the other parts of the cavity.

REMARKS ON THE FOREGOING CASE, WITH SOME OBSERVATIONS ON ANEURISM IN GENERAL.

I have now detailed this unfortunate case faithfully, and not without some hope that it may, on some future occasion, be found useful. Those disposed to be censorious may feel a disposition to charge me with having made some effort to give an undue colouring to my own opinion, because I have made the representation of having had a more correct knowledge of this case than the other medical men who saw it. But I hope a little candid reflection will convince even the censorious, that nothing but a wish to be useful could have induced me to report this case, in which I can claim no merit but that of striving to shed a little light on that kind of aneurism, which the greatest masters in surgery acknowledge to be often enveloped in doubt and difficulty. And I beg leave here to repeat, that no want of discrimination is chargeable to myself, or those who saw so obscure a case. In a person so little observant as to let a disease grow into such force,

as totally to disable him, before it could attract his serious notice, how easily might the most experienced hesitate, and afterwards fall into error. But, as this was more particularly under my care, it would of course become a subject of much reflection, to one who bore so large a share of responsibility; and I would be grossly defective, both in skill and attention, if I had not come nearer a correct view of the case than those who saw it once or twice.

From the circumstance of the limb being above a healthy temperature, when this patient was admitted into the hospital, we ascertain the fact, that the new circulation of the limb had been established some time before. And it is highly probable, that this new circulation would have supported the vitality of the limb, had not the extravasated blood, as an extraneous body, kept up such a state of irritation and inflammation, as to induce a feverish condition of the general system; and such a tumefaction of the member injured, as to subvert all healthy action of the nerves, lymphatics, and in short, the whole economy of the parts below the rupture of the artery.

The following fact, I have to regret did not occur to my recollection till it was too late. In Boyer's Surgery, under the head of aneurism, he relates, speaking of popliteal aneurism, that he "had besides observed on this subject, before the operation, that one of the branches of the superior internal articular arteries was so dilated that its beatings could be felt on the internal condyle of the femur." Might not this be a useful fact to bear in mind. It might tend to throw light on such cases: and, where the new circulation is in a good measure established, and the breach of the popliteal artery below the superior articular arteries, we conclude, *a priori*, that the pulsation of one or more of the articular arteries might be felt, and would be a strong circumstance in favour of the existence of aneurism.

But let us suppose, for the sake of illustration, that we admit that one of the articular arteries had been found beating about the knee; and we had, from hence, been confirmed in our suspicions of aneurism, what practical instruction could we have obtained from such a discovery in the case before us? Could we, under the untoward circumstances which have been detailed

in this report, have practised Hunter's operation, upon the femoral artery, with any good prospect of success? I think not, for in addition to that operation, it would have been necessary to have opened the cavity, and to have taken away the coagula. To accomplish this, it would have been necessary to have made extensive incisions into the ham, and to have carried our incision down through the most fleshy part of the gastrocnemii muscles. Besides, we should most likely run much risk of hemorrhage from the lower portion of the ruptured artery, by a retrograde movement of the blood. These circumstances taken into consideration, together with the ravages which had been made by the presence of the coagula, as an extraneous body, on the muscles, the ligaments, the joint, &c. puts the question beyond all doubt, that, nothing but amputation could have saved the life of the patient, and that, long before he came into the hospital.

John Bell has some severe observations on cases of aneurism, and the reader may imagine this case comes within the pale of one of his *anomalous cases.* But I will fearlessly cite his observations, because I believe his remarks are useful; and because I am endeavouring to render useful, the case before us. He says: "but too often it happens, that when the disease begins in a sprain, when the pain is great, the pulsation small, and the whole member swelled to great size, the general swelling conceals the particular swelling of the aneurism, and the limb is destroyed before the surgeon is aware of the nature of the disease. These are the cases which are so generally called anomalous, or in other words, cases which are not understood. While the surgeon, unwilling to believe that an artery is burst by a sprain of the limb, remains ignorant of the nature of the disease, the artery is actually ruptured, and pouring out its blood among the muscles: the bones are presently destroyed, the whole limb is ruined in its texture, swelled, cold, and lifeless."

Again he says: "when the aneurism happens in the ham, or under the bellies of the gastrocnemii muscles, for by these the aneurism is peculiarly straitened, it is pent up betwixt the hamstring tendons, it forms slowly, is singularly hard, and frequently has neither the pulsation nor other marks of aneurism; but the

knee joint being destroyed, the bones corrupted, and the limb enormously swelled, it requires amputation long before the tumour threatens to burst! But most of all, the limb is in danger when the case is not understood; when the artery is not wounded, nor gradually dilated, but actually burst; for the surgeon, little accustomed to think of this bursting of an artery, never apprehends the true nature of the complaint, nor even knows it to be aneurism!" And further, he says, speaking of the "peculiarities of popliteal aneurism:"—"But in popliteal aneurism, whether proceeding from spontaneous dilatations or not, or arising from sprains, fractures, or wounds, the danger is peculiar; for the aneurism is so confined under the heads of the gastrocnemii muscles, and betwixt the tendons of the hamstrings, that the resistance to its extension is very great, the destruction within is proportionably rapid, and the disease is always attended with severe pain.

"The popliteal artery passes between the condyles of the thigh bone and behind the knee joint, by every motion of which it is affected. It also lies under the gastrocnemii muscles, whence it is endangered by every strain of a limb which is very powerful, and which supports the whole weight of the body. A man, for example, has a fall from his horse; or his limb falls in between the rounds of a ladder, and is violently and suddenly bent, or he receives a blow upon the ham, or he is seized with a sudden cramp in the calf of his leg, and from that moment his pains and lameness begin. Whether the artery be stretched and inflamed, or whether it absolutely gives way under such accidents, is little to the present purpose, but thus it is that the disease begins, and is long unsuspected by the surgeon, while the patient walks, or refrains from walking, by fits, as the pain happens to be more or less violent, rubbing the part perhaps with camphorated oil.

"Thus all parties continue indifferent to one of the most fatal diseases. The ham is straitened, whence the tumour, even from the first, is of a stony hardness. From this straitness it loses very early the marks of aneurism, for the pulsation often ceases, or is obscure. The tumour (even in natural aneurism of the ham) is not always clearly circumscribed, nor to be fairly traced to any connection with the artery; and being covered, and its

pulse suppressed by the great thickness of the muscles and skin, the nature of the disease remains unknown. From the same pressure the leg becomes very early cold and œdematous; for the veins, lymphatics and artery, all pass in the straitened cavity of the ham, the pressure upon these produces a general swelling of the limb, which conceals the particular tumour. From the elevation of the bellies of the gastrocnemii muscles over the tumour, and from the distention of the hamstring muscles, the limb is thrown into severe and painful cramps; and from the nerves passing over the tumour, (pressed sometimes quite flat) a very distressing numbness is always felt, and the lameness and pain are such as the external swelling cannot account for. All these distresses of pain, cramps, swelling and general numbness of the limb, increasing every day, the patient sometimes dies of locked jaw, fever, or long suffering and want of rest, the disease becoming fatal before that period arrives in which the tumour bursts."

I hope I shall be pardoned for making this long quotation from Bell, as, his description is correct and animated beyond a parallel. Now it might be supposed, that a person familiar with the writings of John Bell could not well misunderstand a case of popliteal aneurism. But to the candid who are most profoundly acquainted with such cases, as well in their true character, as in their most insidious disguise, I am confident of an acquittal in the unhappy case I have reported.

I will now briefly condense into view the most material misleading circumstances of this case. First, then, although this patient spoke of unlading the vessel of hogsheads, in which he used considerable exertion; still it was only with a view of pointing out the fatigue which he had undergone, for, he constantly declared, that he had not been sensible of any hurt, sprain, tumour, pulsation, or complaint whatever in his ham, until it had progressed from the foot upwards. Secondly, when he came in, there was no tumour of the ham, in particular, but the prodigious swelling which extended over the whole foot and leg, terminated just above the ham. Thirdly, very soon after his admission into the hospital, the other foot and leg swelled, and became painful, resembling in appearance phlegmasia dolens. This affection of

the right foot yielded in a few days to the application of blisters, over the whole of the swelling; but the cause of this affection is totally unknown.

Now although I am willing to acknowledge that this man must have been mistaken, that he must have injured the artery by a sprain, and that the artery had bursted and poured out its blood; that the swelling and pain of his foot was the consequence of this accident in the ham, still it would have been more than we were entitled, from the symptoms, to do, to insist possitively, contrary to the man's possitive declarations, that it was aneurism. We did suspect aneurism; amputation confirmed our suspicions, and amputation was the only resource left the surgeon, and this after thirty days of extreme suffering, might have been unavailing even then.

It is not because I wish to exculpate myself from a charge of want of skill, that I lengthen out my remarks respecting the obscure nature of my case, but because I think it may be of importance to impress on the minds of young surgeons, that we are not always safe in relying on the most possitive assurances of some men in the common walks of life. And, therefore, I would advise, in all protracted cases, attended with circumstances strong, as were those in this one, to amputate at an early period; for where there is such an amazing extravasation into the ham, which may be inferred from the extreme violence of pain, the stony hardness of the tumour of the ham, and the enormous œdema of the whole limb, nothing but amputation is at all likely to rescue the patient from death.

One word more, and I have done with this part of my subject. Had I been free to follow the dictates of my own judgment, without the risk of public censure, I would have operated, here, at least a week sooner; not because I could in defiance of the man's possitive declarations, that the usual forerunning symptoms of aneurism had not existed, but have concluded that aneurism must exist, because, such a state of ruin of the member affected, the horrible pain, œdema, and total inability to sleep, &c. clearly called for amputation.

I will now proceed to make a few observations of a more general nature, on popliteal aneurism, and wish first, to say something of the reason why amputation is sometimes necessary. John Bell, speaking of this disease says, "it requires amputation long before the tumour threatens to burst;" and further, amputation is necessary, because "the knee joint being destroyed, the bones corrupted and the limb enormously swelled." He continues: "All those distresses of pain, cramps, swelling and general numbness of the limb, increasing every day, the patient sometimes dies of locked jaw, fever, or long suffering and want of rest, the disease becoming fatal long before that period arrives in which the tumou bursts."

In addition to these reasons, and others advanced by Mr. Bell, tending to prove the necessity of amputation, where the extravasation is great, I will now mention what I consider another strong reason, which is, the risk of hemorrhage, from a retrograde motion of the blood, from the lower portion of the artery. These circumstances are, I think, sufficiently momentous to justify amputation, in most cases which have been protracted, till the extravasation is great, and the concomitant symptoms greatly urgent. As regards the operation for this kind of aneurism, I take it for granted, that Hunter's operation is vastly superior to any other; but still in a case like the one I report, we would be under the necessity of laying open the sac, to remove the coagula, and few limbs or constitutions could bear such accumulated sufferings, unless the surgeon were called very early.

It may be supposed by many that the subject of aneurism has been so ably treated on, by so many great masters in surgery, that we have little or nothing more to expect. But notwithstanding the many important observations, and practical lessons, which have been given to the surgical world by Benjamin and John Bell, Monro, Boyer, Scarpa, and many other great teachers of surgery, the subject is only fairly emerging from the obscurity in which it has long been immerged. And however great the obligation, under which we lie, to John Bell for his luminous practical lessons; it is to Scarpa and Boyer, in particular, that we are to look for a rational and correct pathological view of aneurism. To prove

th at the above positions are correct, I shall cite a few observations from the works of these most modern, and highly respected surgical writers—these are John Bell, Boyer, and Scarpa. And it will be admitted, I trust, that the practical rules of surgery are always most safe, where we have the most correct and clear knowledge of the pathology on which they are founded.

Mr. Bell has fallen into an egregious error, in giving a drawing from Walther of an aneurism of the ham, which, conformably to the notions of the old surgeons, is taken to be a true aneurism or a mere dilatation of the arterial coats, or perhaps of the inner coat, forming a cyst, which Boyer resembles to that of an hernia. A more palpable error was never fallen into than was here espoused by Mr. Bell. He says, " the remaining coats of the artery had dilated into a true aneurism, the artery certainly had been injured, though not entirely lacerated, for the drawing represents a true aneurism, a sac entire and continuous with the arterial coats." Professor Smith has remarked upon this drawing, that he thinks Mr. Bell has fallen into error, and he deserves much credit for his good sense in judging for himself, and thus throwing off the shackles of authority, however great, which will not bear the strictest scrutiny.

Now, notwithstanding the greatness of John Bell as a teacher of surgery, I will venture to assert, that he has here, for once, yielded his own experience to the old notions of the nature of true aneurism. From a careful perusal of his observations on aneurism, I have drawn the conclusion, that his own experience and observations are in accordance with those of Boyer and Scarpa. But misled by the many able teachers which have preceded him; and who considered aneurism as formed by the dilatation of one or more coats of an artery, he adopted, in a slight degree, their errors. He seems to have had too much confidence in the anatomical abilities of those great men who so uniformly adopted an error. He should have borne in mind, that a mind intent on confirming what is believed to be true, would be apt to examine slightly. And while they were ignorant of the true pathology of aneurism, and also satisfied with the doctrines generally received, the greatest masters in surgery might readily err, and come un-

der the charge which Mr. Bell applies to dissectors of aneurisms, who, " first" " mangled" " the tumours" " more canine, and then the connections and causes (were) stated in idle conjectures."

In confirmation of this charge against the good sense and profound research of Mr. Bell, I will now cite remarks from his surgery, clearly proving, that, although he admitted the former notions and phraseology respecting aneurism, that he was well aware that it was very easy to run into mistakes, and that false aneurism might be mistaken for the true. Speaking of a case of aortal aneurism, he has these words:—" Here then is the greatest artery of the body burst; fairly torn asunder, and that without any strain or blow. The greatest artery of the body not supported like the femoral artery by a peculiar sheath, nor bedded in firm cellular substance and muscles, but merely covered by the peritoneum, and lodged in the loose cellular substance of the kidney, is yet so supported as to form its aneurism very slowly. The blood is poured into the cellular substance by successive impulses. The sac is fairly circumscribed, and forms an oblong tumour, not much bigger than the fist, though of considerable length, lying close to the side of the artery, and so connected with it as to be cut out along with it : well may such a sac be mistaken for that of a natural aneurism in bungling dissections." And again, " an aneurism which truly arises from a laceration of a wounded artery may be mistaken for a natural aneurism or simple dilatation of the tube."

I can corroborate this last declaration of Mr. Bell, by a brief account of a ventral aortic aneurism of great size; I shall propably give a drawing of this case at some future period. The subject of this disease having been destroyed by it, the late Dr. Middleton, of this city, undertook to prepare the part diseased for the cabinet of the Baltimore Hospital. And I am fully warranted in saying, that, this gentleman, in addition to his amiable deportment, and good sense, should be ranked as one of the best anatomists in this city. But not having paid particular attention to the opinions of Boyer and Scarpa, most probably, or, perhaps, concluding that so much had been said by great men on morbid anatomy, that this case was only calculated to show

us the wonders which nature had accomplished: he made the preparation without having any aim to unfold to view, the true pathology of aneurism.

Having filled the artery of the abdomen, and the sac with the common wax injection, he very neatly dissested off all the cellular and loose membranous matter, throughout the abdomen. We were presented with a tumour which would nearly equal a pint in size; arising from the aorta, with a regular globular form, and looking as if the artery passed through it. This sac has a dense shining appearance, precisely similar to the coat of the aorta, with which it is connected. From this tumour on its left side, and on the most depending part, there has grown out another of about half the size of the first; which appears most manifestly to be formed out of the surrounding cellular substance. In this state of things the superior tumour had every appearance of being a true aneurism, and was generally so considered. I once intimated to a respectable medical friend, that I believed, that if the sac were properly examined, it would be found that the superior sac was formed out of the peritoneum, and to which he pointedly objected. The lateral and inferior tumour appears at first view to be formed out of the cellular substance, and I am persuaded, the old surgeons would have viewed this connective tumour, as the only actual breach of the artery. And this would have afforded a novel and interesting case, being nothing less, according to their notions, than *a false aneurism growing out of a true one.*

Feeling a desire to ascertain the true structure of this tumour, Dr. Dorsey and myself undertook, last winter, to examine more carefully than had been done, into its organization. For this purpose we softened the preparation in hot water, and then made a longitudinal incision the whole length of the sac, which lay exactly in the course of the aorta: and having removed the wax by injecting hot water into the superior sac, we found both ends of the artery distinctly entering into this cavity. The upper portion of the artery, as well within the sac, as throughout its whole extent, is greatly enlarged; but the lower portion of the artery is considerably smaller than in its healthy state. Both portions of the artery retain their cylindrical form, and enter into

the sac like a tube passed some distance into a hollow vessel. The anterior and lateral walls of the sac are formed of a dense shining structure, having a very near resemblance to the arterial coats; or it would perhaps be more correct to say, that, owing to the thickened and condensed state of the arterial coats, no difference can be said to exist. But the lower wall of this superior sac is formed out of the muscles, and of the vertebræ of the loins, the whole of the membranous and cellular structure having been absorbed, as also the principal part of the anterior portion of three of the vertebræ, nearly down to the medulla spinalis. Now had the artery entered into the tumour, in a more indistinct manner, so as to have left any doubt about whether the sac was formed by the dilatation of the arterial coats, the circumstance just related would most clearly prove, that this aneurism was formed out of the surrounding cellular substance. Nothing in my opinion is more extraordinary in this case, than that the peritoneum or cellular sheath, which surrounded the artery, should have formed attachments to the muscles, and the peritoneum of the vertebræ, while the phenomena of the removal of all the parts constituting the bottom of this tumour was going on.

The lateral tumour was formed by the rupture of the left side wall of the superior tumour, but distinctly above the end of the lower portion of the aorta; and its whole structure is formed out of the cellular substance, of much less firm structure than the superior sac. But still we may remark, that notwithstanding the feebleness of the structure of this consecutive sac, it never burst. The patient having been destroyed by a total inability to sleep, for several weeks, and at an advanced age, I ought to have mentioned in the the preceding paragraph that, not only was the flooring, (so to speak,) of the lateral tumour broken up and totally removed; but at least three inches of the aorta was also removed. The ends of the artery, it is true, are ragged; but they are cut off more transversely than one could have imagined. Here then, we must conclude, that the peritoneum, or cellular sheath, or both, having been slowly separated from the artery, the blood has eventually completely insulated the artery, and then, went to work, in the business of removing it as dead matter. We may

here remark, that such was the diminution in the caliber of the inferior portion of the aorta that the circulation through the lower extremities must have been very feeble.

This case is well calculated to deceive; for the superior tumour has all the appearances of what has been considered true aneurism, and the second tumour would have been viewed as a false aneurism, growing out of a true dilatation of the arterial coats. But to show that these deceptive appearances are not uncommon, I will cite a few words from Scarpa. "These appearances the more readily lead us to error, as both the injured artery and the aneurismal sac, as has been frequently mentioned, are covered by a common smooth membrane externally, such as the pleura in the thorax, and the peritoneum in the abdomen."

I shall now pass on to make a few quotations from Boyer and Scarpa, to show that they have had more correct views of the pathology of aneurism, than any other writers with which I am acquainted, and then conclude with a few practical deductions.

Baron Boyer says, "The experience of Haller has been regarded as demonstrative proof of the dilatation of the exterior tunic of the arteries, when their cellular coat is divided or destroyed, and that of the interior, when the last has alone escaped division or destruction. This celebrated physiologist says he has frequently observed in living frogs, that when he raised the membranous tunic of the arteries of the mesentery, and destroyed the cellular tissue that surrounds them, they dilated in the form of aneurism; which was equally the case after an incision, and particularly after a prick through only a part of the coats of the vessel. But whatever skill he may have acquired by long exercise and experiments on living animals, it would be difficult to persuade any one that he could be certain of not having divided the exterior tunic of the vessels, so fine as those on which he operated; it is more than probable, that, in his experiments he divided the partition between the vessels, and that the formation of the tumour he afterwards observed, was only a false aneurism, which gave passage to the blood into the cellular tissue, into which it could only penetrate slowly, by reason of the narrowness of the opening."

And the same author, after advancing many good reasons for believing that surgeons have been mistaken about the nature of aneurism, has these concluding remarks:—"From what we have first said, it may be concluded, 1st, That what has been described as a mixed aneurism, formed by a rupture of the internal and muscular tunics of an artery, and by a dilatation of the cellular tunic, is nothing but true aneurism, at a certain point of development. 2d, That the mixed aneurism produced by the dilatation of the internal tunic, or of this and the muscular tunic forming a sort of herniæ, never did exist."

The same author observes, speaking of true aneurism, that "If a recent and small true aneurism, about the size of an olive be examined, it will be observed, that above and below the tumour the artery preserves its natural diameter. In the tumour the coats are removed from the axis of the vessel, sometimes around the whole circumference of the arterial tube, but most frequently at one point. In this last case, the rest of the circumference of the artery preserves its natural relation with its axis, whilst the affected part discovers, on the inside, a sort of fossa or excavation, that differs from the rest in this circumstance only. The internal tunic extends without interruption over the whole internal surface of this fossa, and the muscular embraces the point, slightly distended, as the rest of the circumference of the artery. In this case there is no rupture, and if the artery, thus dilated, be split, it will be found that the thickness and consistence of its coats, is rather augmented than diminished, and consequently, that the dilatation does not take place at the expense of the thickness of the arterial tunic." Again, "on reflecting on the great number of cases of aneurism, where the proper tunics of the artery have been found destroyed or broken, and the small number of those where the dilatation of the coats has been verified, we are tempted to believe, with Monro, that this kind is very rare." Now I am persuaded, every unprejudiced surgeon, who has carefully perused the works of Bell and Boyer, will be ready to agree with me in the inference, that neither of these great masters in surgery had seen a case of true aneurism of any great size. And

we have just mentioned above, that Monro considered them very rare, and this inference naturally leads me to cite a few words from Scarpa.

Scarpa has examined this subject with the eye of a masterly critic, with a frame of mind philosophically bent on testing what had been said by others by sound experiment and dissection. This state of mind and consequent course of procedure enabled him to arrive at pathological views of aneurism which seem to be absolutely correct, and I proceed to quote a few words merely sufficient to prove that he satisfied his own mind. He says, "This external covering or sheath of the artery actually appears, to those who are not sufficiently skilled in such dissections, as if the artery were dilated under it to such a degree as to form the aneurism; and it has still more the appearance if the aneurism is very large and of long standing, since in this case the cellular coat of the artery becomes unusually thick and pulpy, and because it adheres very firmly to the subjacent muscular coat of the artery at the stricture or neck of the aneurismal sac. But even in these cases, as well as in those of recent and small aneurisms of the aorta, by employing care, we may at least succeed in separating without laceration, this cellular sheath from the tube of the artery, above and below the injury, and successively from the subjacent muscular coat, as far as the neck or root of the aneurism. It is then clearly perceived that the muscular coat of the aorta does not pass beyond the partition which divides its tube from the entrance of the aneurismal sac; and it is distinctly observed, that the fibres and layers of the muscular coat are not prolonged over the aneurismal sac, but terminate like a fringe, or in obtuse points, at the edge of the ruptured artery. On which account, nothing can be more evident than that the aneurismal sac does not belong, at all, to the artery, and that, properly speaking, it is only the cellular sheath, which in the sound state covered and connected the artery to the neighbouring parts, and which being elevated by effused blood, at first in the manner of an *enchymosis*, then distended and compressed, has acquired that degree of density, and of additional thickness and hardness, as if

it had been formed by the proper coats of the artery, prodigiously relaxed, distended, and thickened." And further, he says "the distinction of aneurism into true and *spurious*, adopted in the schools, is only the production of a false theory; since observation shows that there is only one form of this disease, or that caused by a *rupture* of the proper coats of the artery, and an effusion of arterial blood into the cellular sheath which surrounds the ruptured vessels."

In entering upon drawing up practical deductions, it may not be amiss to observe, that the two cases which I have here reported are calculated strongly to corroborate the opinion of Scarpa. And as we may naturally conclude, *a priori*, that practical rules of surgery are most safe when founded on correct pathological views, I shall offer such a nomenclature as, I think, will be useful. In doing this, we remark first, as the terms *true* and *false*, or *spurious aneurism* are erroneous, that we ought to abandon such distinctions, and then we may employ the terms *common aneurism*, for every disease of an artery which has heretofore been called either true or false aneurism. Next we have aneurism from anastomosis, for which see John Bell; and lastly varicose aneurism. Then, I apprehend, we might find the following names well calculated for practical purposes: *common aneurism*, *aneurism by anastomosis*, and *varicose aneurism*.

Common aneurism is always the consequence of a diseased artery, or of an accidental injury; and although we may admit, that the arterial coats do sometimes become thickened, and in a small degree dilated, we contend that such enlargements are most generally, if not always, owing to the thickening of the arterial coats, and that in tumours of any considerable size the artery is ruptured, and the sac formed out of the cellular sheath. From this view we draw the practical inferences, that we should never attempt the cure of this kind of aneurism by compression; that an operation shall never be delayed after we are certain of aneurism; for, however slowly they may sometimes enlarge, there is no security for a single day that blood may not become diffused, from the slightest accident. And the danger must be greatly increased by the extensive extravasation, which

so often takes place. And one principal reason why we should operate early, or that we should not attempt a cure by pressure, is, that an artery seriously injured by accident, or internal disease, must eventually be cut off by ligature; or at some uncertain period it will bring the whole limb, or the whole body, into ruin. And, if we find the tumour large, we may be sure the blood is extravasated; and after tying the artery above we must generally open the sac, and remove the coagula. Where it is in our power, it is always best to tie the artery above the sac, for the artery being always diseased, what certainty have we that it will unite? Such is the situation and structure of the part that, in all protracted cases of popliteal aneurism, where the pain is extreme, we may certainly conclude, that the joint will become anchylosed, the limb contracted and crooked, and the vitality of the leg so much impaired that amputation should unhesitatingly be had recourse to.

Of aneurism by anastomosis I have only to remark, that it seems to be rare; and when met with, the rule laid down by John Bell seems to be all that we can do: he says we are not to cut into such tumours, but to cut them out.

The great success which attends the operation for aneurism in Great Britain, and the U. States, clearly proves that *the ligature of reserve* which the French surgeons have been in the habit of using in their preference for opening the aneurismal sac, instead of taking the sound artery above, has tended greatly to retard the success of their practice.

The success of a case reported by Mr. Crampton, and said to have been successfully repeated by Mr. Astley Cooper, of securing the artery by tapes, while it puts to silence the speculations of Mr. Jones, respecting the necessity of cutting the internal coat of an artery, bid fair at one time to be of vast importance, but now loses all its value, since the discovery of the animal ligature by Dr. Physick.

Of varicose aneurism I have but little to say; if the disease be recent, perhaps, we may succeed by pressure, but it ought not to be long continued. The rules which we would apply to

common aneurism, of operating early, does not apply here. Unless the disease is attended by some unpleasant consequence we conceive it had better be let alone.

ART. VIII. *On Phymosis.*—By J. C. ROUSSEAU, M. D. of Philadelphia.

Avare du sang et ennemi de la douleur.

THE consequences of diseases, and the evils subsequent to many of the adopted modes of cure, are so generally felt that they claim the attention not only of the practitioner, but of all men qualified for beneficial inquiries. Several attempts may prove unsuccessful, but the labours are never entirely lost, and the time spent by the inquirer and the reader is seldom without some benefit.

Several years spent in endeavouring to establish by facts, that almost all venereal affections are but trifling at first, have now gained so many proselytes to this opinion, that it will ere long become a matter of surprise to learn that desolation and death have ever come from such a source; and although a great deal has already been done to remove prejudices and ameliorate the practice on this point, too much still remains to be done to keep us silent. Were the effects of this scourge of the human race confined to those that are held as deserving not only the sufferings of the disease, but the torments of the cure, we might perhaps not feel so ready to sympathise for its victims. But the evil, as we have already shewn, extends further; an error leading to another, a torrent of destructive diseases, under innumerable protean forms, overflows the civilized nations of the globe and cripples their posterities: the causes remain unknown, indeed even unsuspected: the catalogue of diseases is increased and the simplicity of nature entirely overlooked. The genital organs have been and will be at all times, it is perhaps a sorrowful truth, liable to diseases which, after full and fair investigation will, it is

hoped, at no very distant period be viewed in their proper light, and then will no more exhibit those shocking symptoms and deformities, that they are too frequently attended with, at present.

Phymosis is perhaps one of the most formidable of those diseases, and the little of improvement made upon its mode of cure, to this present time, must be considered as the consequence of the slow progress of science, or attributed to servile adherence to received opinions and precepts of the schools.

Indeed it is but little surprising, that hundreds have kept this deformity for life, and suffered the ravages of diseases incident to it, rather than to undergo the process of a terrific painful operation, leaving always the noblest, the most essential part of the animal economy, under such a mutilated state, that nothing less than a complete circumcision, can replace it in a proper shape.

The operation practised for the reduction of the phymosis, attracted my attention several years ago. Doubtful of success, but having then no other alternative, I was under the necessity of performing it. Had any benefit resulted, a degree of consolation to my patient, and a compensation to my wounded feelings might have been afforded; but the evil was aggravated; the disease increased more rapidly: the flaps of the prepuce swelled to an enormous size; the dressings became quite unmanageable, and after a long series of sufferings, an unavoidable deformity was left as a mortifying record of the errors of younger days. To this time the sentiments of horror created on that occasion are recalled to my mind, and raise the keenest sympathy for the sufferer of this modus medendi.

I formed the resolution, but opportunities became rare, to find some better mode of cure, on account of the mode of practice I soon adopted to prevent the formation of the phymosis. No exertions were spared on my part, and the multiplicity of patients falling under my care, at the time I was one of the physicians of the Dispensary, seconded my efforts and favoured my success.

Well aware of the difficulties attending innovations, and convinced that precipitancy had often times raised hopes which were never realized, I could but wish to give a fair trial to my im-

provement, and delay its publication until it was fully tested by experience, and crowned with success. Besides, I cannot conceal it, I was sensible and apprehensive, as it is too well known, that the erroneous effect of fanatical attachment to popular prejudices, might promote censure on my fairest motives; I therefore felt reluctant to offer improvements on a subject contemplated under so many different lights: but as nothing but humane feelings induced me to elucidate it, and considering since that the province of the physician is rather to paliate, than to punish evils brought on by imprudence, and to alleviate instead of aggravating the infirmities of human nature, I hoped my sentiments would be duly appreciated and well received.

A phymosis is generally known to be a contraction of the aperture of the prepuce, preventing its retroversion upon the body of the penis. It may proceed from various causes, and present different indications for its removal. It may be natural or accidental; and, in either case, attended or unattended with disease.

The natural phymosis, until the age of puberty, appears of little consequence; but after this period, for several potent reasons, its reduction is demanded, particularly on account of its favouring or becoming frequently the cause of diseases which might otherwise, be avoided. This alone might be a sufficient inducement, if no other motives of greater concern undoubtedly known to those afflicted with it, called for its reduction. I have had strong reasons to suspect that several cases of marriage were frustrated of offsprings on account of this defect, and in one instance, its removal has confirmed this opinion. The prepuce is oftentimes so long and pendulous, and its aperture so contracted, that not only the ejaculatio seminis, but the flow of urine is impeded in such a manner as to occasion its spreading upon the parts covered by the prepuce, to create an insufferable itching, and, not unfrequently, inflammations and other symptoms that have led to groundless suspicions, and to a very erroneous and mischievous mode of cure.*

* An elderly country gentleman, on account of a phymosis of many years standing, was at last afflicted with ulcerations between the glans and prepuce: the parts having swelled to an alarming size, and the pain become

The accidental or symptomatic state of phymosis, being generally the result of some injury, demands a particular and careful inquiry; it happening frequently without venereal influence, from causes unsuspected by the physician, and unknown to the sufferer; ulceration, itself, not being an evidence of infection.

This, or a disease as similar to this as the organization permits, attacks the sexual parts of females. The labiæ, and more frequently the nymphæ, swell to an enormous size. In few instances I have seen the tumefaction such as to require a great exertion to force the passage of water. This malady, particularly when attended with ulceration, becomes more troublesome and requires more care and attention, although it can seldom admit of any, than that of the male sex; for, in diseases of the female organs, we seldom can apply the proper remedy, for want of being properly informed of, or permited to ascertain the nature of the case. Any affection of those parts is referred, by women, to the gravel, although I may here justly remark that out of a hundred females that consulted me for what they called the gravel, not one was afflicted with this complaint: and as strange as it may appear, I am induced to believe that it is a very rare complaint among women, having not found yet a single case that I could pronounce evidently such, if, as I think it correct, the sole evidence rest upon the evacuation of the gravelly crystals.

In all cases of phymosis the state of inflammation and the degree of tumefaction require the first attention, and it is indispensable to ascertain whether or not it be accompanied with ulceration: for these will most commonly be found proceeding from the acrimony of a puriform discharge aggravated by friction upon parts that have acquired an unnatural degree of irritability; our patients seldom complying with our injunctions, and following carelessly the rules of conduct pointed out for the alleviation of the symp-

intolerable, he mentioned his case to a physician residing in his neighbourhood. The old gentleman, confident of his own conduct, imperatively rejecting every idea of venereal infection, and the physician having not sufficient experience in such cases, requested my advice; he was treated, and cured according to my instructions; abandoning every suspicion of veneral infection, and without resorting to the knife.

toms, before it becomes imperious by the appearance of imminent danger.

It is not very often that we find the tumefaction run *pari passu* with the inflammation; on the contrary, the greater the one, the less the other. The state of inflammation is by far the most dangerous, and with it the ulceration goes on very rapidly.

I have never seen even an extraordinary degree of tumefaction attended with danger. When it happens, the swelling is generally of an anasarcous kind, and occupying the cellular substance of the prepuce, or of the labiæ or nymphæ in the other sex. It appears to be owing to a relaxation of the organic contractility of the fibres, since the collection of serosity resulting from it may generally be prevented by a gentle pressure with a bandage well fixed upon the parts. But if the swelling be already formed, it should be taken down by previously evacuating the watery collections by means of small scarifications made with the point of the lancet, and afterwards applying the roller bandage, tighter and tighter, by slow degrees, at different intervals.

Various remedies have been proposed and put in practice, to reduce the inflammation, but none will prove effectual until the causes alluded to be removed. Mild emollient injections thrown in between the prepuce and glans or between the labiæ into the vagina, by means of a small appropriated syringe, eight or ten times a day, will, not only soften but cleanse the parts from the acrimonious matter, the principal cause of the mischief. External poultices, rest, a proper situation in bed, soft, mild and easy dressings; a cooling but not too rigorous a diet, will soon prove effectual. I have seldom found bloodletting necessary, although it can do no harm in vigorous constitutions; and in two cases threatening mortification, it availed nothing, although it were not used sparingly.

In the various states above mentioned no precepitancy is required; nor is it in the power of the physician to force a cure, which too much officiousness protracts frequently beyond the length of time naturally prescribed.

Removing causes is our sole object; for the phymosis is simply accidental, and in itself no symptom, although it may be the con-

sequence, of venereal infection.* Yet, if it were, its existence could neither prevent nor retard the cure of the pre-existing disease, and the division of the prepuce does not then seem more indicated than would be the incision of the cheeks at both sides of the mouth, to effect or accelerate the cure of an ulcerated sore throat. Indeed the latter operation would in numerous cases often afford a better assistance, than has ever been obtained by the former, if either of them could be supposed to answer any good purpose; for the intention of exposing the ulcers to view and of applying dressings, imagined indispensable, has been generally frustrated on account of the tumefaction either existing, or produced, or increased by the severity of the operation, the extreme irritability of the parts, and many other causes known to those who have either witnessed or performed this operation.

The inflammation being partly abated, moderately astringent injections, free from sediment or muddiness, should be substituted for the former, which, by too long a continuance, are apt to bring on the parts a state of atony very unfriendly to all, and particularly to this kind, of ulcers. The latter kind of injection is also much better calculated to correct the offensive rancidity and the causticity resulting from the peculiar quality and the natural tendency of all puriform secretions, and particularly of those of the genitals under a state of disease.

I have observed that extremely few cases require the sudden and immediate reduction of the phymosis, and in my opinion, and from what I have been taught by the long experience of an extensive and successful practice, it should never be attempted until the prepuce have assumed a yielding appearance. In those cases where an extreme degree of irritability prevails, I have always found better to let it remain undisturbed to the latter end of the cure, unless the edge of the duplicature of the prepuce, or its orifice, be ulcerated; for in such a case, the flow of the urine becomes very unfriendly to the sores, and retards the healing process. Yet, this objection may not be as urgent as might

* I have seen the prepuce swell, in the course of one night, and protruded to three inches beyond the glans, simply from mosketo bites.

be imagined, the inconvenience being in a great measure removed by the ablution of the parts, immediately after the passage of water.

It is, however, next to impossibility, to foresee all circumstances, and, in all cases of every disease, much is always to be left to the judgment of the physician. But too much anxiety must be avoided; for it frequently happens that an hasty retroversion of the prepuce is followed by a still more troublesome and dangerous state, a paraphymosis, more dangerous, more unmanageable and more difficultly removed than the former; threatening mortification if not speedily dilated, and frequently the loss of the glans. And when our patient has escaped all those dangers, we have yet to conduct him through sufferings unknown to those that never had the management of such a disease; and perhaps, after all, be under the necessity of bringing back the prepuce to its former situation, if this can be obtained. For it must be remarked that the glans penis under the pre-existing disease is possessed of a greater degree of irritability than in the healthy state: uncovered, it is left exposed to the action of every thing that comes in contact with it, and the very dressings, used for its protection, are the first to increase the excitement. Besides, the pressure formed by the contracted ring of the prepuce above the corona glandis, keeps the head of the penis under a swollen state, and increases its irritability to the utmost degree; frequent painful erections become the consequence; phlyctænæ are formed on the surface of the glans, and a train of symptoms unknown under the former state of phymosis, supervene and aggravate instead of relieving it.

If under such predicaments the contraction of the prepuce above the head be such as to threaten mortification, no other remedy will afford relief, for a greater hazard is to be run by delay, than the section of the stricture with the knife. In so doing, however, we must take into consideration the future state of the parts, and instead of making a single incision, have a recourse to several. By this method, the stress, which after the section of the ring would be too powerfully felt on that spot, and might produce a further extent of the wound by laceration, increase the

inflammation and favour all its consequences, will be borne and proportionably lessened by the greater number of sections.

This operation completed, the part on which the incisions have been made ought to be drawn back as far as possible, and properly fixed upon the body of the penis; as, in case of erection, the pressure will then be harmless and scarcely sensible to the tender parts of the glans, and the little wounds secluded from the matter issuing from the original ulcers. With this indispensable precaution, they will heal soon and without much trouble, like other incisions made with sharp edge instruments.

If, however, the reduction of the phymosis be determined upon, and every thing promising safety in doing it, tents, of an elongated form and of various sizes, are to be prepared with sponge of a fine texture, free from heterogeneous particles, and very little encumbered with wax or any other plasters.

A small one is then to be passed through the strangulated part of the prepuce so as to reach up to the glans and leave a small portion of it outside: the penis is to be immersed, for a short time, in tepid water, and this bath repeated at intervals of four or five hours, a longer and longer time, at every repetition.

The degree of dilatation obtained by this operation, when the tent is removed, either to cleanse the parts, or to allow the patient to make water, is then to direct the size of the new tent to be used. The moisture in the sponge forming the best application to favour the object, the dilatation will be effected so gradually, that neither inconvenience nor pain will be experienced in the smallest degree; and the same process is to be continued, until the desired end be obtained. Another advantage that will be procured by the sponge is the absorption of the discharge, either from the prepuce or the urethra, which is most commonly the principal cause of this disease.

In some cases of long standing, but extremely rare, an indurated ring, resembling and approaching to the texture of cartilage, very unfriendly to dilatation, has induced me to make a few small incisions with the point of the lancet. Yet, although no visible marks remain after, nor any inconvenience is felt from this operation, patience and time must be fully allowed, before it is resorted

to; the sight of the knife being more terrific and more dreaded, than all the sufferings it is intended to prevent.

The dilatation of the stricture being thus accomplished, the prepuce is to be slowly retroverted and kept stretched as far back as practicable, and without any wrinkles, if possible, upon the body of the penis, until it has acquired its former natural appearance. Cooling lotions are to be used frequently, and every means capable of producing irritation carefully avoided.

I am bold to say, that this mode of cure has never produced any disappointment in my hands, not only during a number of years, but in a multitude of cases.

It cannot, however, be expected that it be unattended with some trouble and difficulties, very little, indeed, being ever obtained without these requisites; but it assuredly saves blood, pain, and deformity to our patients, as it is the intention of the healing art, and is at the reach of every man competent in this business.

It requires, I am sensible, more than a common share of courage and enthusiasm for the advancement of science, to suggest the revisal of old, and favour the adoption of new methods and theories; but we take it to be the duty of every well-wisher to mankind, to neglect nothing that may tend to the improvement of the medical science, whose sole and most desirable object is the alleviation of human infirmities. If this object have not yet been obtained in former, it is to be hoped that in the succession of ages, the degree of perfection so desirable may be attained to, by the exertions and the combined efforts of those in whom the trust is reposed.

In the fulfilment of this duty, we have a just claim to one another's attention and support.

Art. IX. *On the use of Emetics in a certain form of Hysteria.* By John Eberle, M. D. of Philadelphia.

Although the utility of emetics, in a variety of spasmodic diseases, is sufficiently established, by the testimony of many of the most eminent physicians, both ancient and modern, yet, in the

treatment of *hysteria*, they are, as far as I can discover, recommended but by a very small number of writers. I do not find them mentioned as applicable to the cure of this disease in the works of LEWIS, CULLEN, MURRAY, BURDACK, and CHAPMAN, on the materia medica. Besides the testimony of DR. JOS. SMITH,* in favour of the employment of this class of remedies, in the disease in question, I do not, at present, remember any other writer, who has spoken of the use of emetics in hysteria. This gentleman writes: "the experience I have had of the utility of emetics in hysteria and epilepsy, enables me to assert, with confidence, that they are more efficacious, than any remedy ordinarily employed."

It is not my intention, in the present article, to speak of the use of emetics in hysteria generally; I purpose, merely to give some account from my own experience, of their efficacy in a certain distinct and well marked form of this disease.

The form of disease, to which I allude, does not, in its more intense state, differ materially, on a superficial view, from the *Carus* of nosologists; which is defined: *A suspension of the powers of sense and motion, more or less perfect, with an appearance of sleep, the motions of the heart remaining uninterrupted.*† It appears to me, indeed, not at all unlikely, that the ancients described the disease, I am considering, under the name of *Carus*. There is so much confusion in the books, in relation to the soporose diseases, that, we can hardly doubt that they have not been very frequently confounded by writers. This, however, must be a matter of very little moment, since *lethargy*, *cataphora*, *carus*, *&c.* are unquestionably, mere varieties of the same disease, differing in nothing, but in degree of violence. There exists, however, I conceive, an essential difference, between that form of hysteria, which I am about to describe, and these soporose diseases. These affections are evidently connected and dependent on sanguineous congestion in the brain. They depend on the same cause, existing in a less degree, as that, from which apoplexy arises,—pressure

* Transact. of the New-York Physico-Medical Society, Vol. I.

† Young's system of Practical Nosology, p. 248.

on the brain.* Whereas the hysteric torpor, as I shall endeavour to prove, depends on a state of the brain, directly the reverse of that, which obtains in apoplexy, namely, a *deficiency* of arterial blood in the encephalon.

The *hysteric torpor* sometimes comes on suddenly in irritable and nervous females, in consequence of violent mental agitations; sometimes it is preceded and introduced by the ordinary symptoms of hysteria. The patient loses all sensation and powers of voluntary motion; she lies, apparently in a profound sleep, breathes slow and without stertor, or any apparent effort; occasionally draws a deep inspiration; her countenance is pale, the eyes sunk and closed, the pupils contracted, and the mouth generally firmly shut. Occasionally the patient recovers for a moment, in a very imperfect degree, the power of motion and feeling, and relapses again into her previous state of insensibility. During these intervals of a partial recovery of sense and motion, she is observed to press her hands upon her breast, and discovers marks of great distress and anxious feeling in the præcordia. The pulse does not deviate much from its healthy state. In a few cases I have found it small and irregular; though, I believe, it will be generally found full, soft, regular, and a little below its natural frequency. The muscles generally of the body, are somewhat rigid; those of the lower jaw are particularly so. In this, it differs from apoplexy and *carus*, in which the muscles are generally in a complete state of relaxation.† This suspension of the animal functions I have known to continue for more than three days, notwithstanding the assiduous employment of all the remedies, usually resorted to, in such cases.

This form of hysteria, seems to occur more frequently in young unmarried women, than in those of more advanced years. It is generally, I believe, connected with some irregularity in the catamenia. I have known it to supervene on a suppression of the menses from an imprudent exposure to cold.

* Lethargia et carus sunt leviores apoplexiæ species, Mead. mon. et Precept. Med. C. ii.

† Apoplexia per artuum omnium laxitatem distinguetur—Sauvages: Vol. I. page 845.

In looking at a patient labouring under this form of disease, it might readily be taken for a case of syncope, or even perhaps for apoplexy. But if the pulse be examined, it is immediately discovered, that it is not the former; and an attention to the respiration; its freedom from stertor, and the obvious absence of cephalic congestion, at once shows it not to be the latter disease.

The researches of modern pathologists demonstrate, how great a part, local sanguineous congestions perform, in the production of diseases. A loss of balance in the circulation, is regarded by some late writers of eminence,* very justly I conceive, as a principal cause of morbid phenomena. In the disease which forms the subject of this paper, such a loss of balance in the circulation, constitutes I think, a primary circumstance. A congestion of blood in the thoracic and abdominal viscera, though not the immediate cause of the more prominent symptoms of this disease, forms, notwithstanding, the most important link in the chain of cause and effect concerned in its production. But in order that my ideas on this point may be clearly exhibited, it will be necessary to take a more particular and intimate view of the symptoms of the disease in question, and to trace, if possible, their connections and dependencies.

A total suspension of the animal functions, constitutes the prominent and characteristic phenomenon of this disease. All power of feeling and voluntary motion is suspended. But the power of feeling and motion, or in the language of Bichat, animal sensibility and contractility, depend on the regular exercise of the functions of the brain. The brain is the source from which they derive their principle of action. Now, as in this disease the animal functions are in a state of morbid suspension, it follows, that the brain, on whose action they depend, must also be in a state of functional torpor or disorder. This being made out, the next step in this inquiry will be, to endeavour to ascertain in what manner, or by what cause, the functions of the brain become impeded or suspended.

Every one knows that pressure upon the brain destroys the power of voluntary motion and sensation. Every one knows

* Parry, Johnson, Brousais, Armstrong, and Puchelt.

too, that when the animal functions are thus obliterated by cerebral compression, whether that proceed from effused blood, serum, a depressed portion of the cranial bones, or simply from vascular congestion, there are other symptoms which invariably attend, and which distinguish such a state of insensibility from similar conditions not dependent on cerebral compression. Such symptoms are, a very slow pulse, turgid countenance and especially stertorous breathing. But none of these symptoms are present in the disease I am speaking of. Although the patient lie in a state of total insensibility and loss of the power of involuntary motion, yet there is no remarkable slowness of pulse, no congested appearance of the vessels of the head, and no stertor. The suspension of the animal functions in this disease, cannot therefore depend on a compressed state of the brain. There must be some other cause present, acting on the brain, by which its functions become impeded, and thereby those of the organs of animal life suspended. This cause I believe to be, directly the reverse of compression,—a deficiency of *arterial* blood circulating in the brain.

That there is a deficient portion of arterial blood sent to the brain is rendered highly probable by an attention to some of the symptoms. The countenance is pale, the blood-vessels about the head are small, and there is an absence of stertor, which, if congestion existed would hardly be the case. But besides these symptoms which countenance this opinion, there are other considerations, which taken in connection with the other circumstances of the case, appear to me to afford a degree of evidence, in favour of the existence of a diminished state of the circulation in the head, amounting almost to demonstration.

Considering the mass of the blood, as being equally distributed to every part of the system, it is obvious, that, whenever it accumulates in any one portion of the body, beyond its usual proportion, a corresponding diminution of blood must take place in other parts of the system.* Now there are in the disease I speak

* Parry's Elements of Pathology, page 53.

of, the most unequivocal signs of an intense congestion of blood in the vessels of the thoracic and abdominal vicera. Whenever the patient recovers her consciousness and feeling so far as to be able to shew by motions, or expressions the state of her feelings, she invariably refers all her distress to the breast and epigastric region; she feels, as if an immense weight were compressing these parts. Since then there is a large quantity of blood congested in these parts, it is evident, I think, that the usual quantity cannot be sent to the brain.

Having therefore shewn that there is a deficient quantity of arterial blood circulating in the brain in this disease, it remains for me to explain how this deficiency acts in suspending the functions of the brain, and consequently those of the organs of sense and voluntary motion. To those who are acquainted with the writings of Bichat I need not endeavour to demonstrate the close and inseparable relation subsisting between the action of the brain, and the circulation in it, of a due proportion of *arterial* blood. In syncope, the suspension of the organs of animal life, depends without doubt on an interruption of the circulation in the brain. In syncope, there is, however, so total a cessation of circulation in the brain, that even respiration and some other organs of *organic* life receive a temporary interruption. This is not the case in the disease I am discussing. For here, there is still some portion of blood, though diminished from its usual quantity, circulating in the encephalon. Hence the respiration goes on uninterruptedly. Dr. Parry says, "by lessening the momentum of the blood in the brain, you lessen the power of feeling and motion."*

Seeing that congestion in the brain produces an entire suspension of the powers of sensation and motion, it may seem strange perhaps, to some, that a state precisely similar should be produced by a cause directly the reverse of congestion. Strange, however, as this may appear, the apparent opposition vanishes when we come to inquire minutely into the real state of things in san-

* Elements of Pathology, page 297.

guineous congestion of the brain, and in a deficiency of *arterial* blood circulating in the same organ.

In relation to the connection subsisting between the action of the brain and its proportion of arterial blood, these two opposite states of the circulation, namely, congestion and deficiency of blood, act precisely in a similar manner. In both these states, there is a want of a due proportion of arterial blood *circulating* in the brain. This idea I borrow from Dr. Abercrombie* of Edinburgh. To prove, that compression of the brain, in whatever way it be effected, tends to lesson the *circulation* through this organ, we need only take into consideration the relation which the brain as a mass bears to its containing case, the cranium. The cerebral mass exactly fills up the cavity which it occupies. The substance of the brain is incompressible, and the bony case in which it is contained, and which it fills at every point, is wholly unyielding. What must then be the effect, if a greater quantity of blood than common, is driven into the vessels of the brain? The substance of this organ is incompressible; and it is logded in an unyielding bony cavity; yet there is an additional quantity of blood thrust into it—something therefore *must* yield, or the additional mass of blood could not enter its arteries. But there is no part susceptible of such a compression, except the *veins* and *sinuses*. These, therefore, not being able to resist the pressure which is communicated to the general mass, by the arteries, now expanded beyond their usual size by the additional quantity of blood forced into them, are compressed and narrowed in their diameter. The relation between the arteries that bring the blood, and the viens that carry it away is thus entirely destroyed. Hence the blood not being carried away as fast as it is brought, the distention of the arteries becomes greater and greater, and consequently, the veins smaller and smaller, until nearly a total *stagnation* of the circulation is produced in the brain. This being the case, there must be a deficiency of arterial blood *circulating* in the brain, and hence the loss of power. Although in such

* Essay on Apoplexy, p. 16—19.

a case, the arteries contain more blood than in a sound state, yet as it becomes impeded in its circulation, and in a manner stagnates in its vessels, it soon loses its arterial or vital properties. We see therefore, how these two opposite conditions of the circulation in the brain, can produce a similar condition in the functions of this organ.

Believing therefore, that the immediate cause of the symptoms of this disease, consists in a deficient circulation of arterial blood in the brain, depending on a congested state of the vessels of the thorax and abdomen, I would be led from principle, to exhibit emetics for its cure. By the operation of these, the mass of blood which is powerfully congested in the thorax and abdomen, would be dispersed and driven into the brain; and thus, the immediate cause of the morbid phenomena removed. Experience, indeed, has fully convinced me of the usefulness of such remedies, in this form of disease. By the use of an emetic, I have in several cases, recovered my patients from the most profound stupor, in less than thirty minutes. I have generally employed ipecac. in strong doses, though perhaps sulph. zinc. would be better, as it acts more promptly and agitates the viscera more violently.

CASE I.

Some time in February last, I was called to Mrs. Wallace's, No. 303, Race st.—When I entered the parlour, I found a young woman lying on the carpet, in a profound state of insensibility. I shook her, and tried to rouse her, but she remained perfectly insensible to every thing. Her face was rather pale, the pulse was a little fuller than natural, but slower; respiration somewhat heavy, but without stertor. She had lain about fifteen minutes before I saw her. As I could perceive no symptoms of cephalic congestion I advised Mr. Banks, now a student in the Alms-house, to take sixteen ounces of blood—and if she did not exhibit marks of amendment in half an hour, to give her an emetic. She was bled, but did not seem to derive any benefit from it. The emetic was then administered, which operated freely. In a very few

moments after the vomit had operated she opened her eyes an recoved her sensibility. This girl has been subject to hysteric attacks. About a year before the present attack, she had a similar one, in which she had lain above twenty four hours, although under the care of two very intelligent physicians of this city, who treated her by venæ sectio and antispasmodics.

CASE II.

Mrs. Gardener, No. 14, Chester street, a woman of a very delicate habit of body and much subject to hysterical attacks, was taken in July last with the ordinary symptoms of hysteria, such as the globus hystericus, tremours, tightness across the breast, and pain in the abdomen. These symptoms, after continuing for some hours, terminated in a perfect state of insensibility and torpor. She kept her hands pressed upon her breast, her jaws were firmly closed; the countenance pale, pulse small, hard, and of natural frequency; breathing confined, but free from any stertor. Before I saw her she had got asafœtida and æther, but without affording any relief. I immediately gave her an emetic, which operated in about 20 minutes. She recovered her feeling and consciousness so far as to give answers to the questions that were proposed to her. In two hours after, I still found her in a sensible state, though not entirely free from stupor. I ordered another emetic of ipecacuanha, which operating in the ordinary time, removed every symptom of the torpor, but left her in a state of considerable debility.

I might relate several cases more, but these will suffice to show that emetics deserve considerable attention in the disease in question.*

* Although I have not found emetics recommended for the cure of hysteria in any of the books I have consulted, yet it may be proper to state, that in this city, they are not unfrequently prescribed with this intention, by several of our most respectable physicians.

ART. X. *An attempt to shew that the use of Acid Fumigations was known before the time of Morveau, Smyth, &c.* By HENRY W. DUCACHET, M. D. of New York.

A VERY interesting paper might be written upon the pretended discoveries of the present age. It might be shewn, that many of these reputed discoveries were known long before the period at which they are thought to have been made; and that some of them were familiar even to the ancients. It is not my intention to enter fully upon this subject, at this time, but I shall confine my observations to the *acid fumigations* of Guyton Morveau and Carmichael Smyth. Both of these gentlemen claim the honour of having discovered the disinfecting properties of the mineral acids. It is, however, very generally acknowledged, that neither Morveau nor Smyth is justly entitled to the honour they have received; and that acid fumigations were used long before these gentlemen promulgated their experiments.* Dr. James Johnstone, of Worcester, England, published a tract on this subject in the year 1758, in which he informs us of his success in correcting the contagion of a malignant fever, by means of the muriatic acid vapour.† It appears too that the power of the mineral acids to destroy infection, was known to Sir John Pringle as early as the year 1750. But it has never been supposed that the practice was known very long before that period. It will be made manifest in the sequel, however, that acid fumigations were recommended by Boerhaave and Van Swieten; and that they were used even in remote antiquity. Boerhaave, in Aphorism 605, observes that, when any acrid materials are introduced into the fluids of the body by means of any of the non-naturals, they are to be removed or corrected by means suited to their peculiar nature. He enumerates *putrid air* as one of the circumstances which may vitiate the fluids; and recommends for its purification the defla-

* Carmichael Smyth first used them in 1780, and Morveau in 1773.

† See "An account of the discovery of the power of the mineral acids in a state of gas, to destroy contagion," by Dr. John Johnstone, of Birmingham, 1803. Also, "Reply to Dr. J. C. Smyth, 1805," by the same.

gration of nitre, the explosion of gun-powder, the fumes of vinegar, and the burning of marine salt. "Ab aëre acri putrefaciente: hunc emendando accenso nitro, pulvere pyrio, vapore aceti, sale prunis insperso." Van Swieten, in commenting upon this passage, observes, that vinegar evaporated in a flat vessel is an admirable remedy. "Acetum hinc in patulis vasis igni expositum, et in vapores resolutum, summum hic remedium est." In speaking of the use of nitre and the marine salt, he says, that by being sprinkled upon burning coals, they are converted into very subtile *acid vapours.** "Nitrum, vel sal marinus, prunis ardentibus inspersa, solvuntur in vapores acidos mobilissimos, &c. uti chemia docet. Unde statim quaquaversum diffunduntur efficacissimi omnis putredinis domitores." But on the subject of sulphur he is still more particular: and, after exemplifying its wonderful power in destroying the properties of other substances, he says, that the vapours of burning *sulphur* will prevent and arrest all fermentation, and correct all putridity. "Accensi sulphuris vapor omnem fermentationem præcavet, vel natam jam suffocat, atque omni putredini quam maxime resistit."† But as the vapor of sulphur is very pungent, and may be productive of serious injury by inhalation into the lungs, he advises the use of *gun-powder*. This he considers equally efficacious and perfectly safe.

Dr. Mead too, who lived at the same time, was well acquainted with the powers of acid fumes. In his celebrated "Discourse on

* Modern chemistry teaches us that this is not the case. But this does not affect the point we are endeavouring to prove; as it is evident from the passage, that the chemistry of the day taught that an *acid* was produced by the deflagration of nitre, aud the burning of salt. And it is equally manifest, that it was upon *this principle*, the process of disinfecting the air was recommended by the authors I have quoted.

† The following remarkably coincident passage I have discovered, since writing the above, in Boerhaave's Elementa Chemiæ, tom. 2, p. 269. "Vapor ille conclusus cum fermentabili sistit fermentationem. Putrefactionem in omni facile putrescenti cohibet, si modo repitiur gnaviter. Unde et lustrator hic fumus contra *pestilens venenum*, ejus contagium sparsum, aut rebus affixum infectis. Hinc et scitur, cur incensi cum nitro sulphuris flamma, omnium vero maxime pulveris bellici, saluberrimus sit pestilentiæ tempore fumus: nitri, sulphurisque, fulmineus, *totusque acidus vapor, totum aëra emendat.*"

the Plague," he says, " I think it not improper likewise to fume houses with vinegar, either alone or together with nitre, by throwing them upon a hot iron or tile." And again, " the smoke of sulphur, perhaps, as it abounds with an *acid spirit*, &c. may promise some service in this way." Works, p. 212.

It is evident, then, that the power of the mineral acids to correct the impurities of the atmosphere, was known in the days of Boerhaave and Van Swieten, nearly half a century before Doctor Smyth laid claim to the discovery. We proceed to shew that the purifying efficacy of at least one of the mineral acids, was known at a very remote period of antiquity.

In the 16th Book of the Iliad, an incident is related which, we have reason to believe, has reference to a fumigation with the sulphurous acid. When Patroclus, in pursuance of the command of the sage Nestor, is sent to the assistance of the Grecian fleet, the poet tells us, that Achilles, after investing him with his own armour, offers a libation for the success of his friend. For this purpose he took a bowl which had never been profaned by unholy uses, and which he reserved for the exclusive service of Jove; and, as it had lain in his coffer among his garments, he purified it with sulphur.

" Τό ῥα τότ' ἐκ χηλοῖο λαβὼν ἐκάθηρε θείῳ
" Πρῶτον, ἔπειτα δ' ἐνίψ' ὕδατος καλῇσι ῥοῇσι."
ΙΛΙΑΔ. Π. 228.

" This tinged with sulphur, sacred first to flame,
" He purged; and washed it in the running stream."
Pope's Homer's Iliad. Book XVI. line 273.

It is evident from this passage, that the cleansing property of sulphurous fumes, was not unknown even in the days of Homer. Sulphurous fumigations were doubtless commonly had recourse to for the purposes of purification, as we find in the Greek the verb θειόω, *sulphure lustro.* Indeed we learn from another passage in Homer, that they were used for the express purpose of purifying houses. We are told that Ulysses, on his return to Ithaca, after an absence of twenty years, found his palace in-

fested by a number of importunate suitors who continually disturbed the peace of his wife Penelope. Before his arrival was publicly known, he contrived that all of these troublesome visitors should be put to death in his palace. But on entering his house, even before changing his garments, he fumigated it with sulphur, in consequence of its pollution by blood.

> "Bring sulphur straight and fire" (the monarch cries.)
> "She hears and at the word obedient flies.
> "With fire and sulphur, cure of noxious fumes,
> "He purged the walls and blood-polluted rooms."
> Pope's Homer's Odyssey, Book XXII, line 529.

Such lustrations were, no doubt, common in the days of the poet.

ART. XI. *History of an obscure case of Uterine disease.* By THOMAS D. MITCHELL, M. D. of Philadelphia.

Mrs. R—— W——, the subject of the following remarks, was a married woman of delicate constitution of about 45 years of age. She had never borne children, but evinced no sign of uterine obstruction until about two years previous to her final illness. In the year 1817, I was consulted by her on account of a very troublesome fluor albus, in which the discharge, instead of a whitish colour, was generally of a pink hue. This affection was extremely obstinate, but after having resisted a variety of remedies for several months, finally yielded to the tincture of cantharides. My patient, for a considerable time subsequently to this treatment, appeared to enjoy tolerable health, with now and then an occasional period of indisposition, until the commencement of the present year, when she discovered a hard tumour in the right iliac region. Of this she neglected to apprize me, owing to extreme delicacy, until it had existed several months, and had become painful and considerably enlarged. She was frequently seized suddenly with severe paroxysms of pain in the region of the uterus, attended

with vomiting, breathlessness, numbness of the right thigh, and much difficulty in voiding the contents of the bladder and intestines. Her great delicacy recoiled from every attempt to obtain a minute and accurate view of the case, and for this reason, an examination per vaginam, was never made until a short time previous to her decease.

Before the tumour had advanced much in magnitude, evident relief was obtained by rest in a recumbent posture. But her paroxysms were several times obviously induced, by improper exposure in damp weather and by undue bodily exertion. The appetite was generally defective, and became at length so completely impaired, that she seemed to live almost without nourishment of any sort. In the intervals of ease, her health and spirits were tolerably good, and her natural vivacity led her to presume so much on those temporary respites from suffering, as to cause her to be less careful of herself, than her situation required. Her last paroxysm continued for the space of two weeks. It was induced by exposure to wet weather, which renewed her sufferings with an increased violence, that admitted of no abatement or mitigation. Her nights as well as her days, were restless, her whole frame was racked with pain, her stomach almost incessantly contracted with fruitless efforts at evacuation, and the soft parts in the lumbar region burning with a heat that nothing could alleviate. Towards the closing scene, she was occasionally delirious, and on the day preceding her death, became so exhausted as to be unable to converse, or help herself in bed. This exhaustion, however, was attended with considerable mitigation of suffering, and gave to her last hours comparative ease. About an hour before she died, she was conscious of something bursting within her, after which the tumefaction extended to the epigastric region, increasing the vomitive efforts, and breathlessness, and finally terminating in death. Such is the history of this deplorable case.

Respecting the treatment of the case, I am unable to say much that will be likely to subserve the interests of the medical practitioner. My opinion of the nature of the disease was not at all satisfactory to myself at any stage of the complaint. At first and for

several months, I suspected the case to be ovarian dropsy, and treated it with a variety of remedies advised for that affection. But as the tumour advanced in bulk, none of these remedies appeared to be in the least degree useful, until within about two months of her death, when I found not a little advantage from the exhibition of a tonic diuretic mixture, recommended by Dr. John Mace in one of the volumes of the New York Medical Repository. Its active ingredients are iron and squill. This preparation produced an obvious diminution of the tumefaction and an improvement in general health, though the induration in the iliac region remained undiminished. The tumour now appeared to vary its position from right to left, but for the most part was stationary in the right side.

Perceiving that the tumour still occupied its place in the right iliac region, and that it considerably increased, although the general abdominal tumefaction was reduced, I abandoned the idea of ovarian dropsy and supposed the case to be scirrhus uteri. The incessant and intolerable burning in the back seemed greatly to favour this idea, although a usual attendant of that malady, viz. uterine discharge, was wanting. Under this impression of the nature of the case, I regarded it as incurable by any medical effort, and aimed only at palliating the sufferings of my patient.

I had proposed the exhibition of calomel and cicuta, and urged a trial, but the dread of a ptyalism precluded the effort. Laxatives were occasionally used to keep the bowels gently moved, and now and then diuretics were employed, well aware, however, that the increase of the tumour forbade all hope of affording permanent relief in that way. The same physical cause (the tumour) operated also, to keep up the inverted action of the stomach, which could not be relieved by any means that I could employ. The pulse was almost uniformly weak and small; it became rather more active in the last week of her life.

Aware that I could not give any thing like permanent relief, I relied wholly on fomentations and injections. To the internal use of laudanum she had a great aversion, so that I was obliged to administer it per anum. The fomentations of hot brandy and laudanum were sometimes serviceable. Warm water in

bladders and in flannel was occasionally useful, as also the spirit of harsthorn, camphor, &c. I used the warm bath several times, but did not derive from it the advantage I anticipated; the relief it afforded was very transient. Sometimes the liberal use of mustard poultices mitigated the pain, and once or twice the same thing followed the use of a large mush poultice over the abdomen.

Two days previous to the death of the patient, a consultation was had with Dr. Elijah Griffiths of Philadelphia, who joined me in opinion that the disease was scirrhus uteri. The doctor, however, prevailed on our patient to submit to an examination, to which after very great reluctance she consented. The examination per vaginam, though calculated to change our view of the case, rather increased than diminished its obscurity. Low down in the vagina, was felt a large round tumour of great firmness, pressed down into the pelvis almost as firmly as the head of a child in the advanced stage of labour. This was of course the uterus, but the os tincæ was altogether wanting. It appeared to me a firm tumour or thick sac filled with some sort of matter, but having not the smallest aperture. On passing the finger up towards the right os ilium, it was resisted by what appeared to be adhesion of the tumour; on the left side, the finger could be passed rather higher up.

The natural tendency of this examination was to induce the belief, either that the disease was a great enlargement of the uterus, so as to have become a solid tumour, or that it was converted into a thick, tight sac containing blood, pus or some other matter. It was impossible to decide which of these was the fact, because the great tumefaction and hardness in the hypogastric region rendered it impossible to discover a fluctuation, even if it had existed. In either case, however, the power of medicine was clearly inadequate to a cure; and nothing presented as offering the slightest prospect of relief, but an operation on the tumour itself, a measure fraught with uncertainty. It was obvious, that if the tumour should really be a sac of matter, and should burst into the cavity of the abdomen, death must follow. It was equally plain, that the sac might possibly rupture per vaginam, in which case a cure might fairly have been anticipated; but this

was not to be depended on in a case of such importance. It was also imagined that the uterus might have suffered considerable displacement and the os tincæ thus be thrown out of reach of the finger, but not obliterated. This however could not be ascertained, and there appeared to be no alternative but to follow the indication of the examination per vaginam, which was to introduce a trocar into the tumour, for the purpose of evacuating its contents. So great are the liberties that have been taken with the uterus by obstetrical practitioners, that no fear existed about the operation, so far as the viscus itself was concerned. For had the tumour have turned out to be a solid mass, nothing worse could have resulted from the use of the trocar, than might justly have been anticipated from the progress of the disease itself.

Such were the views entertained by us at this stage of the disease. But we did not feel ourselves warranted in proceeding further on our own responsibility, and therefore agreed to call to our aid the combined intelligence of Professors Physick and James. The case having been stated to those gentlemen at full length, they were both of opinion that nothing could be done in the way contemplated, and that it only remained to administer such remedies as might palliate the sufferings of the patient.

From the time of the consultation, the symptoms grew worse and worse. The retching was almost incessant, and the pain excruciating, until one day before her death. She then became much easier, but was almost helpless, having been completely exhausted by the extremity of her sufferings. About an hour before her exit, she was conscious of something having ruptured within her, and this was followed by general tumefaction of the abdomen and softening of the parts in the hypogastric region.

I was extremely anxious to have made an examination *post mortem*, but this desire was frustrated by her friends from a regard to her dying injunction that her body should not be opened. From the fact, however, of the alleged rupture just before death, the evident abdominal enlargement and the solution of the tumour, there is good reason to believe that the case was, as had been imagined, a change of the uterus from its natural form and size,

to a very large, thickened and tight sac, enclosing a large quantity of fluid matter.

Thus have I given a hasty and unconnected history of an incurable disease; more from a desire to see such cases on record for the information of medical men, than from any other consideration. Physicians are too apt to omit the publication of fatal cases; but for myself, I prefer the perusal of such, to the best authenticated histories of successful practice.

ART. XII. *On the Medical Properties of the Secale Cornutum, or Spurred Rye.* By E. A. ATLEE, M. D. of Philadelphia.

THIS very useful article has been introduced to the notice of practitioners in this country, by Doctor Stearns, who, although not the discoverer, yet claims and merits the highest praise for first reducing it to scientific experiment, and publishing to the world the beneficial result of his inquiries.

In the first number of the 2d volume of Professor Silliman's excellent "Journal of Science and the Arts," there is a valuable paper on this subject, by Dr. William Tully of Middletown, Connecticut, in which he has summed up all that appears to be well founded with respect to the article, which he justly considers one of the greatest discoveries of the age. He has denominated it *Scleroticum Clavus*, believing it to be a *parasitic fungus*, like the different sorts of blight, smut, &c. In proof of this opinion of its natural order, he adduces arguments, which although botanically correct, do not require here to be specified.

This medicine, commonly known by the appellation of *Ergot*, appears to exert its power specifically on the uterus, through the medium of what the great RUSH has denominated the "conscience of the body." Its obvious effect is in causing and increasing the *contractions* of this organ, not only "when there is a predisposition to action therein," but when a relaxed state of it requires that these contractions should be excited, in order to its restoration to the proper size and functions.

I shall treat of the Ergot therefore under the general title of *uteri contractor*, and give my views of its efficacy,

1. In accelerating labour.

2. In checking uterine hemorrhage.

3. In obviating other affections of this viscus, depending on morbid relaxation of its fibres.

As a *partus accelerator* it appears to stand unrivalled, notwithstanding the scepticism with which it was opposed by many, of whom I acknowledge myself to have been one, for several years after its introduction. I might have continued through prejudice, a stranger to its virtues until now, had it not been for a distinguished practitioner of this city, Professor Chapman, whom a few years ago I consulted in a case of difficulty, which I apprehended would require instrumental aid. He proposed the Ergot, and notwithstanding I had no confidence in the medicine, I gave it according to his direction, and found, as he had predicted, that in half an hour my patient was delivered of a healthy child and did well.

This circumstance gave me so favourable an opinion of the Ergot, that I determined in future to give it a fair and unprejudiced trial, and I am prepared to unite with many who ascribe to it as direct and certain an operation on the uterus, as the tartarised antimony has upon the stomach, or jalap on the intestinal canal.

My own experience enables me to subscribe, in common with others, to the following indications for its use, viz:

1. In the early stages of pregnancy, when abortion threatens, and has withstood the usual remedies of venesection, opiates, internal and external refrigerants and styptics, when the hemorrhage is alarming, and the contraction of the uterus feeble.

2. In cases of hemorrhage near the end of gestation, when contractions have either not taken place, or are too weak to be effectual.

3. In lingering labours, connected with the death of the child, or in such as are rendered so by a cessation of contraction, the os uteri being well dilated, and a proper relaxation of the other soft parts existing. Such a state of things may take place when there

is no disproportion between the fœtal head and the pelvis of the mother, and where the presentation is most favourable for expulsion. A case of this kind occurred to me, wherein labour had progressed so far as to have ruptured the membranes, when the pains entirely ceased. In this situation I found my patient about bed-time, and having waited an hour, I lay down in an adjoining room, desiring the nurse to call me if pains should return. I was surprised, at being permitted to sleep until morning without disturbance, and on inquiry found, that she had not had a single pain since I had left her. The Ergot was now administered in the dose of a scruple, and before the expiration of half an hour she presented us with a fine son.

But it is more essentially useful in labours rendered difficult by the too great comparative size of the head of the child, or narrowness of the pelvis of the mother, not amounting to deformity, where the pains, although powerful, have effected no more than to push the elongated scalp into the interior strait, there suffering the head to rest for hours, to the manifest danger of the child, and requiring the forceps commonly to complete the delivery. Here it frequently supersedes the use of this instrument, and ought, in every instance, to be fully tried before recourse is had to the iron alternative. And even if the forceps should eventually become necessary, the previous administration of the Ergot prepares the head for the application of it. In this situation of the fœtus, I believe the forceps of Baudeloque is much to be preferred to that of Haighton.

I come next in order to speak of the virtue of our medicine, in checking uterine hemorrhage.

In this case, I have given it about 15 minutes before the probable termination of labour, to a patient who, in her two previous confinements had suffered hemorrhage *ad deliquium*; and its effect answered my most sanguine expectations. Slight hysteria was the only consequence.

In another case of hemorrhage, supposed by my patient to be precursory of abortion, about the second month of pregnancy, which had continued nearly two weeks before I saw her, attended with pain in the back, not at regular intervals, but almost con-

stantly. Apprehending that she had *mistaken* her situation, I prescribed the Ergot in combination with opium, in the following formula:

℞ Secal. cornut. ʒj.
P. G. opii. grs. iiss. M. divid. in chs. x.
Sumat dosin 3tia. quaque hora.

By the time she had taken the eighth powder the hemorrhage ceased; the event proving that she had not been pregnant.

It remains for me to notice the effect of the Ergot, in obviating a peculiar condition of the uterus after parturition, which although but twice occurring in my practice and in but one patient, has not escaped the notice of Burns and others, and must be well known to our eminent accoucheurs. I mean a torpor of the organ after a difficult labour, preventing the contraction of its fibres, the womb remaining for hours, or even days, nearly as much distended as at the full term of gestation; feeling very hard through the parietes of the abdomen, and attended with a pain resembling cramp. A patient of mine, who in three previous confinements had laboured under this painful and alarming state of the uterus, was, in my opinion, saved from it a fourth time by the Ergot in combination with opium. I gave them in the following doses,

℞ Pulv Ergot. ʒ ij.
P. G. Opii. grs. ij. M. divid. in chs. xii.
Sumat unam omni bihorio.

Whether in consequence of this prescription or not, I cannot positively assert, but no symptom of the complaint was manifest, the uterus at the usual period returned to its proper dimensions, the after-pains were moderate, and lochia in proper quantity.

As an emmenagogue I have never used the Ergot, neither do I think that it is calculated to be beneficial as such; unless we admit of a relaxed state of the uterus, causing a retention or suppression of the catamenial secretion, which appears to me rather problematical.

Before I conclude, I will venture an opinion, that in some difficult cases where the Ergot has been administered, the death of the fœtus is sometimes the consequence, owing to the powerful contraction of the uterus separating the placenta before the pro-

per time. I mentioned this supposed effect of the medicine, to two of our very experienced practitioners, Doctors *James* and *Dewees*, who do not agree with me in sentiment. I hope therefore that my apprehensions are groundless on this score, at the same time I believe such consequences probable.

From the preceding observations I infer that the most obvious effects of the Ergot may be traced to the singular power it possesses, of exciting *uterine contraction*, and I am inclined to believe that in all affections of the female (either married or in the virgin state) depending on *morbid relaxation of the womb*, it will be found an invaluable medicine.

ART. XIII. *Some Observations on the employment of Hydro-Cyanic (Prussic) Acid, in the treatment of Diseases.* By BENJAMIN H. COATES, M. D., of Philadelphia, in a letter to one of the Editors.

RESPECTED FRIEND,

AT thy request, and that of Judge Cooper, I communicate the results of a few trials of the hydro-cyanic acid, in the Pennsylvania Hospital, made in the summer of 1819, by direction of Dr. Colhoun.

The acid we employed was made by Frederick Brown, in this city, and was of such strength that one drop, placed upon the tongue of a half-grown cat, destroyed life completely in twenty-seven seconds. Of this, more than two drops, in diluted solution, could rarely be taken without considerable inconvenience, in twenty-four hours. In two or three weeks, however, it would lose much of its strength, and then we generally replaced it by a fresh parcel.

Of pulmonary consumptions, we had none in a stage sufficiently early to test the powers of the medicine; so that in several trials made, it appeared to do no good. But its most remarkable effects were exhibited, when employed in cases, in which I have never read of its having been ever previously used.

Being considered as so powerful a lessener of irritability and

sensibility, it was given with this view in nervous pains and in mania.

Mary Berrian, aged about forty years, of a spare, delicate habit, had been for five years constantly more or less affected with rheumatism, and pains consequent to it, which would appear to be purely nervous. She had been salivated three times, and taken a great variety of medicines, with very great, but not total relief; and previously to her admission into the hospital, she had remained for a considerable time without any improvement. She now suffered so much pain in her limbs, that she was obliged to take large doses of laudanum every night, although she had no marks of fever. Her skin was cold and shrunk, her pulse small and a little accelerated, her appetite defective, and her whole appearance that of a person debilitated by long continued disease, and by depleting remedies. She was put upon a light, nourishing diet, and her opium continued; and on the eighth month, 5th, 1819, was given as follows:

℞ Acid hydro-cyan. gtt. ij. Aq. fluv. ℥viij. m. Capt. cochl. maj, i. 2nda quaque horâ; and next day her laudanum began a gradual diminution. By this she was entirely freed from pain in two or three days, and in a great measure so in the first twenty-four hours. The pains, however, soon returned, but with the assistance of a few very gentle antiphlogistic means, they finally disappeared in a few weeks, and she was discharged cured. In the eleventh month she had a slight relapse, and was treated by diaphoretics, without any benefit. I have never heard of her since.

Angelina Brown, near middle age, had been afflicted three months with rheumatism in her foot, with a small degree of inflammation; and three or four weeks with rheumatic headach. She underwent a gentle depletory course, with tr. opii gtt. 100, every night, and blisters. These gave a slight relief, but her distress was very great, and she could not sleep without the laudanum in the dose of fifty drops. Seventh month, 27th, she was given—℞ Acid hydro-cyan. gtt. j. Aq. ℥viij. m. capt. cochl. magn. 2. 2nda quaque horâ. On the 28th, two drops were put into ℥viij. and on the 30th, she was directed to use one half the mixture every day. With this, slightly varied, and a few subsidiary means,

generally an occasional bleeding, purge, or blister, she recovered in three or four weeks, and was retained as an assistant to the nurse of her ward.

G. B. aged nineteen years, admitted 8th mo. 4th, with mania of a very violent grade, took acid hydro-cyan. gtt. ij. of the best, in ℥viij. of water; in the dose of ℥ss quaque 2nda horâ. It was increased to gtt. iv. but he could only take of this ʒj. to ʒiij. every two hours. This remedy, in conjunction with copious bleeding, purging, and blistering, and with some opium, was thought to have a considerable effect in quieting and composing this very distracted patient. It was continued till 8th mo. 22d; after which, a continuance of the before mentioned means, to a greater extent, succeeded by a salivation, effected a complete cure. He was accordingly discharged, 11th mo. 16th, 1819, and is supposed to have continued well.

ART. XIV. *Case of periodical discharge of Blood from the Rectum of a Male.* By P. S. TOWNSEND, M. D. of New York.

CHRISTAIN ANDRES, a Hollander, æt. 72, was, thirty-nine years ago, attacked with a discharge of black blood from the anus, which continued from three to eight days. This discharge has supervened at every *full moon* since that date, and occupies the space of time which it assumed at the first evacuation. It is unaccompanied by pain. He is a seaman by profession, and was seized with this affection at Curracoa. Hooper in his Medical Dictionary, article *hæmorrhois*, refers to a similar case.

September, 1819.

ART. XV. *A Case of Polypus Nasi completely eradicated by the use of Scotch Snuff.* By P. S. TOWNSEND, M. D. of New York.

As the use of snuff has generally been supposed to be one of the many causes of this disease, it will be interesting to the medical

world to be informed of the facts detailed in the following case, and which lead us to hope that one species of snuff at least will prove to be one of the most invaluable remedies for this malady ever yet discovered

October, 1820. Mrs. S—— S——y, of Orange county in this state, was seized with this complaint about twenty-six or twenty-seven years since. The polypi were of the ordinary species, and appeared in both nostrils. Before they had arrived to any great magnitude, they were removed by Dr. Richard Bayley, formerly health officer of this port. After a lapse of fifteen years they again appeared, and increased to an alarming size; enlarging the cavities of the nose, and protruding upon the upper lip; affecting also the adjacent parts of the face, eyes and forehead with severe pain and considerable tumour and inflammation. It was now thought that the disease would take its ordinary course, and end in the destruction of the surrounding bones and cavities, and the death of the patient.

Having accidentally heard at this time, through her neighbour, a Mr. Bradley, that he had been cured by the use of scotch snuff, she had immediate recourse to it in large quantities, so much so, that, during the first few weeks, it caused her to vomit severely, by which a great deal of matter was discharged and great relief experienced. The nostrils were at this time so plugged up by the polypi, that she was under the necessity of throwing her head back, in order to introduce the snuff into her nose. The snuff was afterwards used in the common way, and without intermission. The tumours at the same time began to recede, and finally diminished so much as to retract themselves entirely within the nares. The pain and swelling in the nose, face, and eyes, also gradually subsided, and these parts assumed their natural appearance; so that in a few years every vestige of the disease disappeared, and her health and vigor were entirely restored. Her countenance is now perfectly natural, her voice clear and distinct; and all her features have the proportions and regularity of health.

The snuff should be of the strongest kind. I have since attended another patient who is rapidly getting rid of this disease by the use of the same remedy. Professor Mott, to whom I mentioned

this remedy, has, I have been informed, lately introduced it into his practice with advantage.

ART. XVI. *An extraordinary Case of Scrotal Hernia.*—Communicated by Drs. CHARLES MARKLE and JEREMIAH FOOTE, of Bedford county, Virginia.

ON the second of August last, we were called to visit a negro man about forty years of age, who had been afflicted with inguinal hernia for several years, though at times the intestine had descended nearly into the scrotum, but could generally be reduced with but little difficulty, except a hard tumour directly over the ring where the pad of the truss was applied. On making inquiry, we found the circulation of the fæces had been obstructed ten days, during which time every important exertion had been made to effect a reduction, but in vain. The family had administered cathartics and injections, which only increased the painful state of excitement, without having any effect to draw the intestine from the stricture. His pulse had become weak and intermitting; his countenance dejected; his hair wet with perspiration; the surface of the body cold, yet moist. But notwithstanding these, and other corresponding symptoms of strangulation, we judged by the appearance of the intestine that it could not have been long completely strangulated. We soon concluded on an operation, as the only means of relief; and on exposing the displaced intestine in the common manner, we found attached to it a tumour of the size of a hen's egg; colour, shape, and consistence, much resembling the gizzard of a turkey. This tumour was slightly attached to the intestine by thin, loose adipose membrane, by a very broad surface, which we separated with the handle of the scapel, till we found a very firm connection of one edge, by which the tumour was nourished. Fearing consequent inflammation, we dissected no farther, but after dilating the ring sufficiently, we returned the intestine, together with the tumour. After closing the integuments by suture, adhesive straps, and proper bandaging, we directed a common dose of the oleum ricini, and a free use of sweetened

gruel. In about three hours the alvine discharges were restored, attended with but little pain, and ever since he has been regular in his bowels, his appetite and strength gradually improving, and in three weeks after the operation the wound nearly healed, and the negro able to walk about the plantation; since which time, we are informed, he is in good health, and free from any inconvenience from the disease or operation.

What we consider peculiarly extraordinary in this case, was the tumour, which we presume must have been caused by the pressure of the truss on a protruding edge of the intestine, which of course must have never been within the abdomen previous to the operation. The size of the tumour required considerable dilatation of the ring, which we were happy to find was followed with but a moderate loss of blood. We were also much gratified to witness so sudden and happy relief from the most alarming and threatening symptoms, which existed in this case prior to the operation.

ART. XVII. *A Case of Congenital Rupture of the Abdominal Integuments at the Umbilicus.*—Communicated by W. BRADLEY TYLER, M. D. of Fredericktown, Maryland.

MRS. D. was delivered of a full grown child, on the 11th of August, 1820, after an easy and short labour. On separating it from the placenta, its appearance shocked and alarmed all present; medical aid was promptly called for. I saw it in ten minutes, and found a rupture of the abdominal integuments at the umbilicus, with a protrusion of the entire intestinal tube, through an orifice of about three quarters of an inch in diameter. The child was alive, and presented in other respects, every aspect of health. The intestines lay fully exposed on the abdominal surface, and were free from disease.

I attempted a reduction of the parts, but found the opening too contracted for the safe return of so great a length of tube. The only mode of surmounting this difficulty, which occurred to me,

was to enlarge the ruptured orifice, by a free incision of the integuments, and thus to provide the requisite space for the restoration of the protruded parts. This was proposed, but obstinately resisted by the parents of the child. Its success was rendered very questionable, apart from the danger from inflammation, &c. consequent on the operation; for on further investigation, the cavity was found so much reduced from contraction and adhesion, as to be utterly insufficient for the accommodation of the protruded mass. I returned about four inches of the tube, which sensibly affected the powers of life, particularly the respiratory function, from the pressure on the diaphragm. The circulation became languid, and instant death seemed alone prevented by a withdrawal of the replaced bowel. The child lived thirty hours; gangrenous spots were soon manifested, and at length spread over the whole surface.

Mrs. D. furnished the following probable explanation of this case. She stated, that in April last she was thrown from her horse, and fell with great violence on the abdomen; and that immediately after, she suffered much pain and soreness, which continued in some degree until her delivery.

ART. XVIII. *A singular Case of Ulcerated Stomach, unattended, during life, by any symptoms of disease in that organ.*—Communicated by JOHN C. CHEESMAN, M. D. of New-York.

ON the 1st of June, 1819, a gentleman about sixty years of age, who had led a remarkably regular and temperate life, was attacked with a peculiar uneasiness in the abdomen, which was thought to arise from irregularity of the bowels. A variety of evacuant medicines were used, but gave him no relief. Fomentations and anodyne embrocations procured only temporary benefit. He continued in this state for about a month, with little or no variation, when he was suddenly seized with a violent pain in the superior part of the rectus abdominis muscle, extending down to the umbilicus, and inclining to the left side. In consequence of

the violently spasmodic affection of this muscle, a variety of anti-spasmodic medicines were directed, but with very inconsiderable advantage. Bloodletting, warm bathing, &c. were productive of no better effect. It may be proper to state, that this gentleman had, for several years, suffered occasionally from a rheumatic affection of the right leg, thigh, and hip, which had produced considerable lameness, and greatly impaired his general health. The disease under which he now laboured, was thought to be of the same nature; and as all the ordinary remedies had failed in affording alleviation to his distress, he was advised to try the effect of sea-bathing. This remedy was faithfully had recourse to, but without any advantage to the local affection, or any improvement to his general health. He was gradually exhausted by this continual irritation, and died on the 30th of August, 1820.

DISSECTION.

Upon raising the abdominal parietes, a firm adhesion was found to exist between the peritoneum and the convex surface of the liver, immediately under that part of the rectus muscle in which the pain and spasm had been seated. The superior portion of the stomach also was completely united to the concave surface of the liver. The stomach was then particularly examined; and on its external surface was discovered *an ulcer* of about three inches in diameter, the edges of which were high and much jagged. Intus-susception was found in several portions of the intestinal canal.

The intus-susceptions may account for the strange uneasiness which the patient experienced in the early stage of his disease, and the unusual adhesions which have been described, may explain the pain and spasms of the rectus muscle; but there was no symptom whatever which indicated disease of the stomach. A variety of opinions were entertained respecting the nature and precise location of his complaint; but not one of the medical gentlemen who visited him, had any suspicions of an affection of the stomach.

REVIEWS.

Quidquid venerit obvium, loquamur
Morosa sine cogitatione.

MARTIAL.

ART. XIX. I.—*Report of a Committee of the Board of Health of the town of Boston, appointed on the 30th of August, 1819, to investigate and state all the facts relative to the ship Ten Brothers.*

II.—*A series of Letters and other Documents relating to the late Epidemic or Yellow Fever, comprising Essays of the Physicians, &c. &c.* 8vo. pp. 208. *Baltimore*, 1820.

III.—*Report of the Committee appointed to investigate the causes and extent of the late extraordinary Sickness and Mortality in the town of Mobile.* 1820.

IV.—*A historical sketch of the Endemic Yellow Fever, which prevailed in the city of New-Orleans, in the summer and autumn of* 1819. By DUPUY DE CHAMBERRY, M. D. &c. (New-York Medical Repository, October, 1820.)

V.—*A letter on the Yellow Fever of the West Indies.* By DANIEL OSGOOD, M. D. Practitioner of Medicine in the city of Havanna. pp. 72. 1820.

THERE is no point in Pathology, which it is more desirable, in this country, to settle, than the prominent and distinctive characters of Yellow Fever. The march of knowledge is always slow; and there is nothing by which her progress is so often, or so ef-

fectually retarded, as the reverence with which we are generally disposed to view long received opinions. What men have long heard confidently affirmed as truths, they are apt, at last, to regard as axioms; and to receive with great hesitation and suspicion, any opinions or facts, which go to invalidate them. To diffuse more just opinions, on such subjects, requires, not only strong and clear evidence, but it is necessary that this proof should be frequently exhibited in various points of view, and after all, it must be contemplated for a considerable length of time, before it will be generally well received. But, " truth is great and will prevail." Her progress, though slow, is sure; and after her lineaments have been once fully made out, and her identity clearly ascertained, she is received with confidence, and there is little danger of her being afterwards forgotten or abandoned.

If this be true in the most common and familiar concerns of life, how much more strongly is it illustrated in every department of medicine. Our art is surrounded by so many out works, its approach is rendered difficult by so many obstructions, and there is so little about it, which the great majority of mankind find attractive, that but little is known respecting it, out of the very pale of the profession.

Even among well educated men, at this day, when it is the fashion to know every thing, is it considered as no reproach for a man to display the grossest ignorance of the most common and obvious doctrines in medicine. There is only one way in which we can hope to rectify the opinions of mankind, on those points where the general diffusion of just opinions is of importance to the welfare of society;—and that is for physicians themselves to investigate, with diligence and honesty, the evidence which is brought forward on such subjects, and to endeavour to make a just estimate, arising from the resemblance of the thing itself, and the well known laws of the animal economy. In doing this we should take care that we do not seek to confirm our present belief, that we do not argue for victory; more especially should we be careful, if we conceive our interest or reputation in any way concerned in establishing one side of the question.

In this manner we may hope at last to establish just views, and

a uniformity of opinion among the members of the profession, and afterwards to diffuse these opinions through the whole mass of society.

The feature of this disease which is most interesting, and which it is desirable most clearly to develop, is its origin. We are conscious that this is a subject which has been of late so often discussed that it has become trite, and that it will strike upon the ear of many of our readers, "like a thrice told tale." We are perfectly sensible that the members of the faculty, in this country, are daily becoming more orthodox on this point. Already, a great majority of the physicians are convinced, that the remote cause of this disease is one of the products of chemical action, and not of animal secretion, that it is most frequently of domestic origin, and is not communicable from a sick to a healthy body. We have even been informed, notwithstanding the extraordinary regulations of the Board of Health of this city, in relation to Baltimore, during the summer of 1819, that the physicians of the Board, at least a majority of them, were perfectly satisfied of the non-contagious nature of the disease. Still we feel it our duty to do our part in following up the advantages which have been gained, and contending still more earnestly in the cause of truth. This is not one of those speculative questions which sometimes occur in pathology, the discussion of which is chiefly valuable as it serves to sharpen the faculties and develop the powers of the human understanding, but which may be settled either way without materially affecting the happiness of mankind. Whatever may be the prevalent opinions of the faculty, we know that these opinions have been but little propagated, among the people at large. We know that, at this moment, the laws of every civilized nation are founded on the assumption of the contagious nature of this disease. We know that there are men, who, influenced either by an extraordinary incredulity, and dulness of apprehension, by an affectation of eccentricity, pride of opinion, or even more unworthy motives, remain deaf to the immense mass of unquestionable facts and unanswerable arguments which have been brought forward on this subject, and are still endeavouring to confirm the preju-

dices of the people with all the activity, and in the true spirit of proselytism.

But do not let us be misunderstood. We should be sorry, in discussing a question of science, to seem to indulge a sectarian spirit. In a question of as much importance as the present, one may be allowed to be warm, but we would wish to avoid being uncharitable. We therefore modify what has been said by remarking, that, although it is entirely incomprehensible to us, yet, we admit that there are some, nay, we know personally at least one, of whose acquaintance with the subject, intelligence, and purity of intention we entertain the highest respect, whose creed is opposite to our own. But the fact of the existence of such persons, we would urge as our excuse, for occupying the time of our readers on the present occasion, in entering pretty fully into the consideration of this question.

We will therefore, without further apology, at once endeavour to ascertain whether, from the evidence which has been given to the public by competent and creditable witnesses, concerning the disease denominated yellow fever, which has recently appeared in various part of the West Indies, and United States, there is good reason to believe that it was in every, or any instance, contagious.

The summer of 1819, will be long memorable on account of the great extent to which this disease prevailed in most of the principal cities in the United States. We shall begin with Boston, in which the disease presented itself in a very interesting point of view, and under very peculiar circumstances.

About the latter part of June, and beginning of July, several well marked cases of yellow fever were observed, near the same spot where it first appeared in 1798 and 1802. About a month after this, on the first of August, the ship Ten Brothers went up from quarantine ground to the town. There is direct and unquestionable evidence, " that a great number of persons were on board this vessel after her arrival, and while her cargo was unlading, and the great majority of them have remained in health. But among them a number, how many is not known, but it is believed about 12 persons, living in various parts of the town, were

seized with a fever of a very bad character, and almost every one so seized, died."

The history of this ship is briefly this. The Ten Brothers, which was on a trading voyage to the coast of Africa, arrived at Prince's Island on the 19th of January, 1819, all well. It soon after became very sickly among the shipping in the harbour. By the middle of February, the captain of the Ten Brothers died, and the crew being very sickly, Captain Cobb, who succeeded to the command of the ship, very properly put to sea, to recruit his men, and this was attended with the most favourable effects. He, afterwards, returned to this island, received into his vessel 1800 bushels of corn, and arrived at Martinico on the 29th of June, with his crew in health. Here he disposed of his corn, took in stone ballast and sailed for Boston, and arrived on quarantine ground on the 26th of July. The most laudable precautions seem to have been taken by the captain, in the cleansing and purification of his ship. During the time the ship remained at sea, she leaked considerably, but, after her arrival, while she lay at quarantine, but very little. She remained in quarantine for five days, and all her crew being in fine health, she then went up to town, on the first of August. "After Captain Cobb left quarantine, before the vessel arrived at the wharf in Boston, the ship laboured much, and the crew were wet and much exhausted." None of the crew assisted in unlading her, but they all slept in her, the night that she came up to the wharf. They were all taken sick the next day, and three died in five days. "On Wednesday, after the ship arrived at the wharf, and when unlading her, a white scum was discovered on the bilge water in her lower hold, and an unusual stench from the water pumped out of her; putrid corn was likewise discovered scattered in her timbers and under her ceiling."

Can any satisfactory solution be given of this uncommon and interesting case? It is evident that the destructive poison, imbibed from this ship by so many persons, must have been received in one of two ways. It must have been either some subtile, morbid poison, or contagion, existing in the vessel, derived from the bodies of those who had sickened and died of yellow fever, dur-

ing the time the vessel remained in the sickly atmosphere of Africa, or it must have emanated from the putrid corn and condition of the ship.

Supposing the first to have been the case: It will be recollected that there was no sickness on board the Ten Brothers from the first of May, when she returned to Prince's Island, to the first of August, with the exception of one sailor, " who, from great fatigue and a violent cold, taken during a heavy tornado which they experienced during the passage to Martinico," sickened and died.

We must then suppose that there was excreted from the bodies of the sick a poison which became attached to the vessel, and insinuated itself into the trunks, clothes, &c. which remained latent, and innocuous, for three months, and which was not removed by the judicious and extraordinary exertions, which were employed during that time, to purify the vessel. But the moment the vessel comes along side the wharf at Boston the spell is broken, this fatal enemy of human life assumes all its malignant properties, and the first victims are those who have been steadily exposed to its effects for three months! Of course we should suppose that every individual seized with this dreadful malady would become a centre from which this pestilence would radiate, and extend its deadly influence. But we learn, " that this was not the case in a *single instance!*"

Will the second supposition more satisfactorily explain the phenomena observed? We may suppose that the corn, which had insinuated itself between the timbers and ceiling of the vessel, remained perfectly harmless so long as the vessel was at sea, when, " in consequence of leaking considerably," the putrefied corn was washed over several times in a day with clean salt water, and the injurious gases, which were disengaged, were immediately diluted with pure air, which would pass freely to every part of the ship while her hatches were open, and she was moving through the water. As soon, however, as she was anchored at quarantine, we learn she leaked but little. She lay in this manner exposed to the heat of the sun for five days, in the latter part of July, in an unusually warm season. " After she left quarantine, and before she arrived in Boston, the ship laboured much,

and the crew were much wet and fatigued." The agitation of the bilge water was favourable to the evolution of any gases formed by the putrefaction of the corn. The night of her arrival at the wharf, the crew, wet and exhausted, slept in the vessel enveloped in this noxious atmosphere. Is it a forced or improbable supposition, or in any way contrary to our experience, and knowledge of the effects of certain agents, evolved during the process of vegetable putrefaction on the human body, to impute the morbid phenomena which ensued to this cause?

At any rate, on the question of the contagious character of the disease, the evidence is direct and unequivocal. We are informed by the committee, whose business it was to inquire into the subject, and who seem to have performed their duty with great fidelity and ability, " that they had not heard of a single case where the disease had been communicated by the diseased patient."

We shall next examine the " Series of letters and other documents relating to the late epidemic or yellow fever as it appeared in Baltimore," during the summer and autumn of 1819. This volume, chiefly, consists of letters written by the physicians of that city, in answer to certain interrogatories of the Mayor, put by him for the purpose of taking precautions for preventing the recurrence of a calamity, from which the city had suffered so much. As these letters were not originally intended for the press, but were permitted to be published by their authors, for the purpose of assisting the Dispensary with the funds that might be thus raised, it would not be fair to submit them to the test of criticism, as literary compositions. There are several of these letters, however, which, even in this respect, are very creditable to their authors. But the work will be considered chiefly valuable as a record, from the number and respectability of the witnesses, and the great mass of well authenticated facts which are brought forward to illustrate some of the most important features of the disease.

There are letters from twenty-two physicians, and it is agreed by all, without a dissenting voice, that the disease confined its

ravages to low, moist, clayey situations. Not a single instance is mentioned where it occurred in the dry and elevated parts of the city, in which it could not be unquestionably proved, that the individual had exposed himself by visiting those districts in which the disease was known to prevail, and the atmosphere to be polluted with the specific cause of yellow fever. It was likewise agreed among these gentlemen, that they did not know of a single instance in which there was reason to suspect that the disease was communicated from one person to another, though numerous instances are narrated where persons, who had become sick in the unhealthy parts of the town, were removed into the healthy districts. During the prevalence of the disease, several hundreds of persons were removed from the infected districts, and encamped on an elevated and healthy spot in the neighbourhood of the city. Among these were, of course, many persons who had imbibed the seeds of the disorder, previous to their removal. We are informed by Dr. Townsend, the physician at the encampment, that "out of the whole number which occurred there, several were permitted to remain; during which time, no attempt was made towards a separation of the sick from the healthy; on the contrary, a free communication was permitted in all parts of the camp. Strangers were daily visiting it, and yet not a solitary case was known that could not be clearly and unequivocally traced." In the hospital there were likewise, during the whole course of the disease, *constantly* present from thirty to fifty patients, in every stage of yellow fever; and although there were about one hundred and thirty other patients in the same house, and uninterrupted intercourse kept up with the other parts of the building, yet not in a single instance was the disease propagated either to the other patients or to the attendants.*

There is probably no town in the United States which has suffered more, in proportion to its population, in one season, than Mobile. It is scarcely possible to imagine circumstances more favourable to the development of the disease, in its most ma-

* Vide our last April Number.

lignant and destructive form, than is stated, by the committee appointed to inquire into the affair, to have existed there in the summer and autumn of 1819. Some idea may be formed of its shocking ravages from the following facts. It was supposed that soon after the disease began, the population was reduced to about five hundred, a considerable portion of whom resided in the suburbs, where the fever did not prevail. The total number of deaths, from July the 4th to December 1st, was about 274. The following is described by the committee as the condition of the wharves: "They were built of hewn timber, closely laid, confining the water within the dimensions of the wharves, and filled up with rotten logs, bushes, shavings, and other vegetable matter, covered lightly with swamp mud, or earth, presenting an immense mass in the most noxious state of decay." "When the committee viewed these wharves (in winter), the sight was most disgusting, and the smell so offensive, that they considered their health endangered by delaying about them. Not only the wharves, but a considerable portion of the town, is taken from the bed of the river, and constructed of similar materials." When to this is superadded "the most abominable filthiness in the public streets and private inclosures, and a total neglect of all those wholesome municipal regulations, which have been found indispensable for preserving the health of a large number of persons congregated together, and this too in a warm climate, in lat. 30° 40′, we think the most sceptical will allow, that the causes at home were amply sufficient to account for all the effects produced. Yet there were persons in Mobile "who strenuously maintained that the fever was imported." The vessel charged with importing this morbid poison was the Patriot, which arrived at Mobile from the Havanna, on the 19th of August, "which was the only vessel which arrived from the West Indies during the months of June, July and August." But the disease had then been in existence a month, and several persons whose names are mentioned by the committee, had died long prior to this period with all the diagnostic symptoms of yellow fever. The committee unequivocally declare "that from the most diligent inquiries"—"they are constrained to express their decided conviction that the malignant fever, which so recently afflicted

our town, originated in the numerous causes they have mentioned." "It was observed that the suburbs of the town, at no greater distance than a mile from the river, were as healthy during the prevalence of the fever, as more distant parts of the country; *and it is not known that the disease was communicated in any instance to persons out of the town by the removal and attendance of the sick.*" The committee remark that the disease at the same time prevailed as high up the Mississippi as Natchez, and Baton Rouge, and on the waters of the Tombeckbee and Alabama rivers; particularly at Fort Jackson, Fort St. Stevens, Fort Claiborne, &c. places entirely disconnected with commerce, and so remotely situated, as to be inaccessible to foreign vessels.

Not a summer passes but the yellow fever appears in New Orleans in a greater or less degree. This will probably always be the case, from the latitude and locality of this city. Its ravages, in common seasons, seem to be confined to strangers, particularly those coming from high latitudes. In the summer of 1819, it was peculiarly malignant; natives and strangers were indiscriminately its victims. It was observed by a medical gentleman, who was on the spot at this time, and who has very strongly and feelingly depicted the characters of the disease:* "On a subject so much discussed, and respecting which there is so much difference of opinion as the contagiousness and origin of yellow fever, my opinion would carry but little conviction; but I cannot help expressing my opinion that it is the bilious remittent and intermittent fever, made more malignant by circumstances, and not *contagious*."

Dr. Chamberry, who seems to be a pretty extensive practitioner in New Orleans, and to have taken an active part on the present occasion, has given a short, but lively and interesting account of this epidemic, in the last number of the New York Medical Repository: we acknowledge we should have felt more confidence in it, if there had not been quite so much "studying of effect." We confess that in scientific subjects, we cannot hear men speak in round numbers, on such topics as are mentioned in the follow-

* New York Medical Repository, January, 1820.

ing paragraph, without feeling a little uncomfortable. But still the remarks are interesting, and very much in point, in our present inquiry, and the testimony, so far as we know, unimpeachable.

"I formerly believed the yellow fever to be contagious, but since I have been in the midst of it, my numerous practical observations have never been able to furnish me with one proof of this much dreaded attribute. Indeed the result has been quite the reverse; and I am now convinced, that the disease is permanently fixed to the spot, and within the limits of the place which has created it. Not one case occurred beyond the limits of the city, during its prevalence in the year 1817 and 1819, that could be traced to any of our innumerable patients, although daily intercourse was kept up with people of the neighbouring estates and plantations. A great number of our inhabitants who carried the seeds of the disorder abroad, seeking refuge from the danger at a distance, suffered an attack of the fever and died; but in no instance was it communicated to their friends. *Fifty times* have I had my hands and face besmeared with the putrid blood, black vomit, or fœtid slimy matter of perspiration. Fifty times have I been immersed in the effluvia issuing from a dead or living subject, and never been infected by the disease. From extensive observations, I infer, that the yellow fever of this place is a disease, sui generis, the product of local causes, and neither contagious, nor exportable. Flight from the infected spot is the only preservative, and a distance of three miles appears to be quite sufficient to inspire the fullest confidence."

It was not our intention to have gone beyond the United States for authorities on the present occasion. But the circumstances of Dr. Osgood having been a native of this country, and educated in it, his vast opportunity of observation, and the recent publication of his "letter" in New York, render him a very suitable and valuable witness on the present question. This subject is just touched upon by him in passing, but his testimony is nevertheless decided and strong. He observes: "Nor have I seen an instance, during nearly twenty years constant practice in this disease, (the yellow fever of the West Indies) in which it appeared to

me to be communicated from one person to another, or from its having originated from any exposure whatever, excepting in places peculiarly circumstanced."

It will be proper to subjoin the following extract from a report made by Drs. Hewson, Chapman, and Jackson in August last during the prevalence of yellow fever in Philadelphia.

"It may serve to allay some part of the dread, incident to this terrible fever, to state that not the slightest evidence of its being propagated by contagion has come under our notice, and moreover that the whole of our patients, without a single exception, have confessed an intercourse with the infected spot, which is now ascertained to be exceedingly circumscribed."

We have thus gone over a very extensive field, and passed under our review the most authentic accounts which we can find of the yellow fever, as it has appeared during the two past years on a tract of country extending from the Gulf of Mexico to the 42d degree of north latitude. We have devoted more of our time and space in our Journal than we can well afford, in order to decide the question of the contagious nature of the disease, so far as depends upon its history, in so many of the cities of the United States, in one year. In order to prevent all cavilling or suspicion that evidence has received any colouring from us, we have selected only such witnesses as have had the best opportunities of investigating the fact, concerning which they have testified, and have quoted their own language. To do away the charge, that these witnesses belong to a party in the profession, we have always chosen those who were disconnected with it, when we considered them equally worthy of belief. It is proper to remark that these witnesses have not been brought forward by us from our previous knowledge of their opinions, but we have taken them, indiscriminately, as we have found them. In fact the essays, enumerated at the head of this article, include nearly all that has been recently published on the subject.

To the credibility of the witnesses, therefore, we are sure no objection can be made, and we have no doubt but that its credibility will be universally admitted, and that, according to their knowledge, they have spoken not only the truth, but the whole truth.

The laws of nature are constant and uniform; they are not directed by caprice nor liable to change; this is as true of the morbid affections of animals as of the other changes which are observed to take place in nature. As the form of the human body, its stature, the period of its growth and decay, are precisely now what they were a thousand years ago, so we have reason to believe, that all the essential characters of disease remain unaltered. Fortunately but very few diseases are endued with the remarkable power of reproducing themselves. If this property were common to all, man would soon be exterminated from the face of the earth. There is no disease which is universally acknowledged to possess this quality which has ever been known to be produced by any other agent than this specific contagion. Smallpox, measles, syphilis, hydrophobia, &c. have never been known to arise from any modification of heat and cold, dryness or moisture, or accumulation of filth, but, whenever they have been seen, could be almost uniformly traced to their specific contagion. It has been asserted by some that yellow fever is an exception to this rule. Although it is admitted that it frequently originates from inhaling an atmosphere polluted with the various gaseous products of putrefaction, yet it is affirmed that after it has once been excited in this way, it acquires the extraordinary property of propagating its kind. To say nothing of the fact, that this character has not been observed in any other disease, and the strong presumption that it is not the case in this, arising from analogy, let us suppose for a moment that this is the case. It seems to us a stretch of credulity, that no sane man will submit to, to suppose that a disease can at one time be so contagious that it may be conveyed for hundreds of miles concealed about the person of an individual in perfect health, and then left to infect a whole community, while, at another, it is so entirely destitute of this property, that one hundred and thirty persons may inhale for months, an atmosphere loaded with exhalations arising from the bodies of from thirty to forty persons, sick and dying, under every possible form of the disease, without its being communicated in a single instance. That it may prevail in a small district of a city, containing 60,000 souls, (no means being taken to

prevent the sick from being removed from the infected to the healthy parts,) and yet not a single instance occur where the disease was propagated: that, in the course of two successive seasons, it should have made dreadful havock in seven cities of the United States, the aggregate amount of the population of which is nearly half a million, and yet not a single clearly established case of contagion be discovered: that, according to the written testimony of more than thirty intelligent and regularly educated Physicians, practicing in places where they had become familiar with the disease, they had never seen a case where they believed it was contagious. We can only say, from what we understand of the rules of evidence, and according to our power of estimating it, that we can hardly imagine how a stronger case can be made out. To be sure there is one spot where the disease seems to be a little more compliant. We can imagine only one reason why it should so completely change its character on this spot, and that is, that it may be the means of adding another title to the President of the New-York Historical Society.*

The advocates of contagion seem to act on the principle that, if they can make out one strong case of the disease being propagated in this manner, their point is established. But to us this opinion seems very incorrect. It has been very justly remarked, that " observation or experiment can determine with much greater certainty *what does not*, than what does give infection." Such are the inaccuracies to which all accounts of diseases are liable, and so innumerable and various are the sources of error to which they are exposed, arising as well from the misapprehensions and misstatements of the patient, as from the prejudices of the practitioner, that a few insulated cases, however apparently well substantiated, ought never to be considered as decisive in giving a character to a disease. The facility with which this has heretofore been done, has tended, perhaps more than any thing else, to bring the profession into ridicule and disgrace. An instance oc-

* Vide process verbal, for installing the President of the New-York Historical Society, &c.

curred during the last summer, which is so much in point in our present inquiry, and is so strongly illustrative of this principle, that although we are conscious of having already trespassed too long on the patience of our readers, we cannot refrain from mentioning it. It will be remembered that during the past summer several of the towns bordering on the Connecticut river, were alarmed by the appearance of yellow fever. We have understood from good authority, that the following circumstance took place: A boy had been on board the vessel which was supposed to have imported the contagion. A short time after he sickened and died of symptoms of yellow fever. Soon after this another person in the same family, sickened and died of similar symptoms. This was immediately seized upon by the contagionists as an unquestionable example of the truth of their doctrines. On investigation, however, it appeared, that another person had died with similar symptoms, in the same family, a considerable time previous to the arrival of the vessel in the river. This we think shows in a striking manner how little reliance can be placed upon a few extraordinary cases in settling a question like the present.

From all the evidence which we have examined on the subject, we are inclined to believe that yellow fever is excited by a modification of the same cause which produces intermittent fever. There is, however, one fact, on which we have often reflected, and concerning which we confess our entire inability to give any satisfactory explanation. It is this; in the north and eastern parts of the United States, particularly in Rhode Island, Massachusetts, New Hampshire, and Maine, for 60 or 70 miles from the sea coast, the intermittent fevèrs are unknown. Now when it is recollected how prevalent these fevers are in the fenny districts of Lincolnshire, in England, and in Holland, and their fatal effects upon the English army on the Island of Walcheren during the year 1810, although these places are in a much higher latitude, one cannot help believing that something else is necessary besides heat, moisture, and vegetable matter, to the production of this disease; some peculiar property of the soil perhaps. We should be glad to see this subject more thoroughly investigated. To such an exactness have the manipulations of chemistry arrived,

that it seems to us, by no means incredible, that some method may yet be devised for obtaining this poison in a sufficiently concentrated form to ascertain with accuracy its precise nature and chemical properties. Nor does it appear to us at all chimerical to suppose, that some mode may yet be devised for neutralizing its deadly properties, an event not more improbable than the invention of the safety lamp.

Since the above was written we have met with Dr. Watts's "Account of the yellow fever which appeared in various parts of the United States during the summer and autumn of 1819," in the New York Medical and Surgical Register. His object is much more extended than our own, and from the very able manner in which he has conducted every part of this "Account," he has in a great measure, superseded the necessity of the present inquiry. We were at first inclined even to suppress what we had written, but on further reflection have thought that although we are conscious of having done, much less successfully, what he has done so well; still that it would have a good tendency in rectifying public opinion on this subject, to bring into a focus such a body of respectable witnesses on this very important subject. We have been much gratified by the marks of patient investigation, and the plain practical good sense with which this memoir abounds. We were especially pleased with the elevated and manly sentiments avowed by the writer in speaking of the disgraceful affair of the passengers in the sloop Union. We know of nothing which is better adapted to increase the rank and respectability of the profession than the general promulgation of such generous and high-minded sentiments. We have likewise been pleased to find that we have so high an authority as that of Dr. Watts for asserting that "a single case, a more than doubtful instance of contagion, furnishes the miserable amount of evidence of the contagiousness of yellow fever derived from the epidemic of 1819 in New York."

We are sensible that some apology is due to our readers for having extended this article to its present length. We can only plead our conviction of its great importance in a national point of view, in our defence. Not a summer passes but we hear of

some one of the principal cities in the United States, and sometimes all of them being visited by this fatal and loathsome pestilence. The moment this is known, the story is repeated and re-echoed from one newspaper to another throughout the country, and swells in importance at every insertion. Immediately this spot is considered as contaminated. The large cities pass their decrees of pains and penalties against its citizens, which are responded to by every insignificant village. All intercourse between such city, and the rest of our common country is interdicted; its inhabitants are at least for a time completely excommunicated, and the rites of hospitality denied them. A knowledge of this soon finds its way into foreign countries, where the geographical situation of the different cities being but little understood, the whole country is considered polluted with some horrible pestilence. The consequence is, that the most vexatious and ruinous restrictions are imposed on all intercourse with every part of our country. This must necessarily operate as a serious check on the enterprise of individuals and the commercial prosperity of the nation. Now it is manifest if there be any means of preventing the approach, or circumscribing the ravages of this disease, and the injurious consequences which follow in its train, that this can only be effected by ascertaining its nature and origin, not by hypothetical reasoning, but by a reference to facts which rest on such authority as will not be questioned.

ART. XX. *The Philadelphia Journal of the Medical and Physical Sciences.* Edited by N. CHAPMAN, M. D.

AMONG the new publications which have appeared since our last number, the above Journal, from the manner of its being ushered into the world, and from the high auspices under which it has appeared in the literary horizon, is particularly calculated to attract our attention. The avowed object of the Philadelphia Journal is "to evolve and stimulate the genius of the country to invigorated efforts, *by holding out a respectable and more permanent*

repository for its productions;" to stand forth the champion of American science, and to retrieve the medical literature of the country, from the contumelious sneers and aspersions which have been so illiberally cast upon it. For the successful performance of this undertaking, all the talent of what has been considered the first school of medicine in our country, was emphatically and loudly pledged. We confess that we looked for the first number of this Journal with an interest, and impatience, which we have seldom experienced.

We should not think, that we were discharging our duty to our readers, should we pass over this publication, without giving them some idea of the manner in which these high promises have been fulfilled, in the first number. Indeed, on perusing it, we have found so much to which we are disposed to call their attention, that, if we were to indulge our inclinations in this respect, our remarks would occupy two large a portion of that department of our journal which is devoted to these inquiries.

The prospectus, which is placed as a preface to the Journal, is written with great care and considerable talent. In the selection of its topics, the writer has been judicious; but there is something, in the management of them, which we cannot approve. There is a constant appeal made to popular prejudice, an effort to conciliate public favour, at the expense of truth and independence, a frequent reiteration of " unrivalled " excellence of our countrymen, in every qualification required in a physician, &c. &c. Now we should not be surprised to see all this in a political composition. We know that there are men who rise into an ephemeral distinction, by artfully playing upon such feelings; and who owe their eminence, mainly, to their unprincipled address in this respect. It would not excite our surprise, or any other sentiment but pity, to find such a person, at one time speaking of Great Britain, in terms so hyperbolical, that even Englishmen would blush at the exaggeration and grossness of the flattery, while, at another, he would exhaust invective, and be unable to find terms of reproach sufficiently bitter, in speaking of the same people;—if we should find him at one time discoursing of his country, as if all the worth and talent, and virtue in it, were

confined within the narrow limits of a small political sect, and that all the other parts, including its government, were only remarkable for their baseness and imbecility, and, in a short time afterwards, courting this very government, by every act of self abasement, and by every fawning, servile art, which an ignoble mind can imagine. We say, that these things would not excite our wonder, and scarcely our indignation, should we witness them in a political demagogue, because we know that these are the arts which such characters employ to mislead the judgment, lull asleep the suspicions, and steal upon the confidence of the people. But when we see the editor of a Journal which is dedicated to science, and the avowed object of which is to elevate the medical character of the country, stooping from the dignity of his station, and seeming to depend for success, not upon its intrinsic excellence, but upon his address, in practising upon the infirmities of mankind, we cannot avoid expressing our decided disapprobation.

There are none, who feel more indignant at the base calumniators of our country than we do. We believe we feel an interest as deep, an affection as ardent, and a pride as sensitive in every thing which relates to the true dignity and welfare of the country, as those who talk more about it. We will yield to none in our admiration of the structure of our government, and the energetic character of our countrymen, and we most sincerely believe, that there are none, who anticipate, with more confidence, or more fondly look forward to our future literary greatness, than ourselves. Nevertheless we do think, that the proper way to hasten and effect this object is not to tell our countrymen that they have already arrived at perfection, or to flatter their national vanity by saying to them that they have done more than any one else, and therefore, that but little remains to be done.

Since, however, the editor of the "Philadelphia Journal" is so zealous a stickler for "American genius," since he is so anxious "to invigorate its efforts by holding out a respectable and more permanent repository for its productions," we would inquire how it happens that, on one occasion at least, he seems to have been utterly insensible to its claims. It is a fact too notorious to be soon for-

gotten, that he stands charged before the public, with being a virulent persecutor of one of the best men and most distinguished citizens, which this or any other country has produced. That a person should have been active in impugning the genius of the illustrious RUSH, undoubtedly the first physician that has yet appeared amongst us, and whose name has been long consecrated among the benefactors of his nation and his age, and then talk about his anxiety "to evolve the genius of the country," seems to us an instance of inconsistency that cannot well be reconciled.

We trust that the independence with which we have spoken, and shall continue to speak of this and every other production, concerning which we shall undertake to pass judgment, will not be mistaken. We hope, we shall not be suspected of allowing narrow jealousies, personal pique, or any unworthy motives to influence, or seduce us from exercising our editorial functions with integrity. On examining our minds, we do not perceive in any manner, this to be the case, nor can we doubt that the peculiar and independent relation in which we stand, will totally exculpate us in the minds of our readers from such a charge.

We shall, on this, and every other occasion, exert our limited means to awaken and keep alive, a spirit of free inquiry, to encourage merit, and foster genius. On the other hand, while we studiously avoid any wanton attack upon the feelings of another, we shall not hesitate to explore hollow assertion and empty pretence, whenever we find it; nor will we allow the delinquent to escape, whether he be found with a cap and bells, or under the grave and imposing robe of a professor.

We shall now proceed to examine the scientific part of this Journal; the first article of which is entitled,

"*On the nourishment of the Fœtus. By* N. CHAPMAN, *M. D.*"

This article, we perceive, is a lecture, which the author has been in the habit of delivering to his class for several years past. About three years ago, it was read, as we are informed, before the Philosophical Society of this city; and during the last summer four or five abortive attempts were made, to convene the me-

dical committee of this association, for the purpose, as the members were informed by special notification, of hearing Dr. Chapman read his paper on the nourishment of the fœtus. It was finally read to a few of the members of that committee; and now at length, we find it gracing the front of the "Philadelphia Journal of the Medical and Physical Sciences."

We mention these circumstances, in order to show with what fondness and pride, the author cherishes this new embroidered bantling, and how evidently he seems to regard it as one of the most happy offsprings of his intellect. We must confess, however, that we see neither comeliness in its form nor vigour in its constitution; and, as in fact, it is the legitimate offspring of another gentleman, we cannot help expressing ourselves a good deal displeased with Dr. Chapman, for having shown himself so bad a *foster-nurse* in the present instance, since, what was originally a fine and sensible little urchin, he has now turned out into the world, "to bide the peltings of the pitiless storm," a pale-faced, sickly and distorted brat, whose tinsel trappings, serve but to excite our commiseration.

He begins by referring all the various speculations respecting the nourishment of the fœtus, to the two following heads:

1. That the fœtus is sustained by the liquor amnii received by the mouth.

2. That it derives its food through the umbilical vessels.

The first of these doctrines has been conclusively refuted by Alexander Monro the elder, nearly a century ago, and our author, accordingly, very properly rejects it. We do not find that he has brought forward any new arguments on this point, in the present instance. Having dismissed this doctrine, he enters upon the consideration of the second one just stated, and after adducing a variety of arguments, in opposition to it, he rejects that also.

Although it be now almost universally addmitted, that the fœtus receives its nourishment from the mother, through the medium of the umbilical cord, yet the particular circumstances connected with this phenomenon are by no means settled. By some it is

believed that the arteries of the maternal portion of the placenta secrete a nutritious, chyliform fluid, which is taken up by the umbilical *veins*, and carried to the fœtus, for its nourishment. By others, it is maintained, that the blood of the mother constitutes the fœtal nourishment, and that this fluid is conveyed directly from the vessels of the womb, by those of the placenta and funis, to the fœtus.

The first of these doctrines, Dr. C. rejects only in part; for he admits, that the "vessels of the placenta secrete a fluid for the nourishment of the fœtus;" but he does not believe, that this fluid is taken up by the extremities of the umbilical veins, and by them carried to the fœtus. The latter opinion he rejects altogether, and brings forward arguments against its reception. We shall presently examine the force and bearing of these arguments.

Admitting therefore, as he does, that the arteries of the placenta secrete a chyliform fluid, "highly elaborated, and adapted to the nourishment of the fœtus," how is this fluid conveyed to the fœtus, if the veins do not perform this office? Upon this point, the author says: "To me it is manifest, that the fluid is taken up by a set of absorbents, opening into the cells of the placenta, which run along the umbilical cord and terminate in the liver of the fœtus." This is the *sum total* of the professor's doctrine (for we will for the present call it his);—a doctrine upon which, if we are to judge from the great anxiety he has shown to bring it forward on all occasions, he seems to rest no trifling claims to reputation, and by the publication of which, he no doubt hopes to dissipate the contemptuous interrogatories contained in his motto, from the Edinburgh Review.*

In reading this article, we are struck with the evident anxiety which the author betrays, in almost every paragraph, to impress his readers with the belief that the doctrine he advances is original with him. That he is not, however, entitled to the least credit, for originality in this instance, is readily shown; nor will it be more difficult to show, that his arguments are altogether

* "In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture? what does the world yet owe to American physicians and surgeons." *Edinburgh Review.*

futile, and his doctrines untenable. In the second volume of the "*Medical Essays and Observations published by a society of physicians at Edinburgh*, 1734," there is an essay on the nourishment of the fœtus, by Dr. Alexander Monro, the elder, in which all the leading arguments and doctrines brought forward by Dr. Chapman in the article under review, are explicitly and unequivocally set forth. We will now give extracts from this essay in proof of this assertion.

After adducing some very common place arguments, against the existence of a continuous circulation of blood, from the mother to the fœtus, our author says: "Driven forward by the energies of her (the mother's) heart and arteries, the stream of blood urged in this way, could not fail, at once to crush and reduce to chaotic mass the exquisitely delicate and tender organization of the embrio, or even of the more advanced fœtus." Dr. Monro says: "'Tis worth while to remark, by the way, the inconveniences that are shunned by the want of an anastomosis between the vessels of the womb and secundines. The violence of the mother's circulatory fluids are not in hazard of destroying the embrio while tender."*

Dr. Chapman asserts that the red blood of the mother, is not carried, by the vessels of the funis, to the fœtus; and, that its nourishment is derived from a fluid of a lactescent character, secreted by the placental arteries. "I cannot help thinking," says Dr. C. that *I am entitled to the conclusion*, that the fœtus fabricates its own blood, and that the only dependence, which it has on its mother, is for a supply of materials, out of which it is formed. Does any thing more take place here, than in the egg? Confessedly the chick by its own organs, produces the blood, and what reason is there to doubt, that the fœtus in the viviparous animals, which has an equivalent apparatus, is not competent to the same office?" Monro† says: "The red particles of blood are not probably absorbed; my reasons for thinking so, are: the chylous appearance of what is separated by the glandulæ of cows, and sheep; and the want of an example of red globules being absorbed any

* Vide Medical Essays and Observations, vol. ii. p. 135.
† Ibid. page 144.

where else. If it be asked, whence then has the *fœtus* the red blood? I answer, without entering into any philosophical comparison of the *placenta* in the fœtus, and of the lungs in respiring animals, that fœtuses of viviparous animals have their red blood from the same source that chickens *in ovo* have theirs; which can have no other, than the action of their heart, and of the vessels in their body, and secundines."

Dr. Chapman's remarks on the analogy between the nourishment of the fœtuses of viviparous and oviparous animals, is an abstract of Monro's chapter "*On the nourishment of the fœtuses of oviparous animals.*"* We desire the reader to examine for himself, as it would extend this article beyond due bounds, were we to give the passages here.

The great idea, however, which constitutes the bone and marrow of this doctrine, so ardently advocated by our professor, is, *that there is a set of absorbents in the umbilical cord, which, opening into the placenta, take up the nutritive fluid secreted in that organ, and which convey it to the fœtus, to be elaborated into blood.* This idea Dr. C. has claimed as original to himself, both in public and private, in his lectures and in conversation, as we have ourselves repeatedly witnessed; and yet the following passage must have been perfectly familiar to him, since he refers upon other occasions to the very essay from which it is extracted.

Having shown, that the red blood of the mother does not pass into the fœtus, but that there is a lactescent fluid secreted by the placental arteries, Dr. Monro says, "to shorten hereafter the dispute concerning the nourishment of a fœtus, I may remark, that it will be sufficient for my purpose in the present question TO HAVE PART OF THE VESSELS OF THE SECUNDINES GRANTED TO BE ABSORBENTS, the negative of which, I dare affirm, nobody will undertake to prove."†

We can perceive no difference between this opinion, advanced by Monro, and the one offered, in the present essay, by Dr. Chap-

* Medical Essays and Observations, vol. ii. p. 204.
† Ibid. vol. ii. p. 144.

man, as his own. Why therefore did not Dr. C. refer to Monro, as the source from which he drew this idea? He cannot plead in excuse for this flagrant dereliction of his duty, that he was not aware of what Monro had said on this point; for we have positive evidence that he had read this writer's observations upon fœtal nourishment.

But Dr. Monro is not the only writer who has published this doctrine of the nourishment of the fœtus. Schreeger, a German physician, in an academic dissertation, entitled *De functione placentae uterinae*, Erlangen, 1799, of which we have a copy, delivers a doctrine, not as Dr. C. says, very *analogous* to the one he discusses, but precisely and identically the same. There is even very little difference between the arguments of those two gentlemen in support of this doctrine. Schreeger maintains that "there is a fluid of a sero-albuminous nature separated by the uterine arteries, which is deposited into the cells of the placenta: From these cells it is absorbed by absorbent vessels of the umbilical cord, and by these carried to the thoracic duct, which conveys it into the blood-vessels of the fœtus, where it undergoes the process of assimilation, by the action of its own organs. It is again returned from the fœtus to the placenta, and after undergoing the process of sanguification in that organ, it is returned to the fœtus by the umbilical vein, and serves to support by its nourishment the tender fabric of the embryo. The recrementitious parts of the blood are rejected by the umbilical arteries into the spongy tissue of the placenta, and afterwards removed by the lymphatics of the womb."*

Dr. Chapman says, in the concluding paragraph of his paper, "It appears from a note in the *last* edition of Richerand's Physiology, that a German writer of the name of Schreder (Schreeger) entertains views of this subject very analogous to those I have advanced. Whether he has priority of claim I am unable to determine, as the date of his publication does not appear. I have taught the doctrine in my lectures since 1803, and read a paper on it at Edinburgh *two* years before." Schreeger published his

* Vide Schreeger de functione Placentæ Uterinæ Dissertatio, 1799.

essay in 1799. Of this fact, Dr. Chapman was informed above a year ago. Dr. Chapman evidently wishes to produce the impression, that the note in Richerand's Physiology, referring to Schreeger's doctrine, was first published in the *last* edition of this work. "From a note," says he, "in the *last* edition of Richerand's Physiology," &c. &c. Dr. Chapman ought to have acted more candidly, and stated that this note was published in *all* the editions of Richerand *ever* published. Dr. C. must have known this fact, for he himself edited one edition of this work; and it is to be presumed that he read the work as soon as it was published in our own language.

In Hopkins & Earle's edition of this work, 1808, which was republished in this country from the *first* English translation, made from the *first* French edition in London, 1803, Schreeger's ideas on the nourishment of the fœtus will be found, not *in a note*, but in the body of the work. How then can Dr. C. honestly say, "*from a note in the* LAST *edition*," &c.?

We will not accuse Dr. C. of having read the original work of Schreeger, for beside that that book is extremely scarce in this country, it is printed in the *Latin* language.

Having now, we conceive, proved that Dr. Chapman has advanced nothing new in the paper before us, we will enter into a cursory examination of the validity of some of his arguments.

It appears to be established, upon the strongest evidence possible—direct experiment, that there is no continuous vascular connection between the uterus and the fœtus; or in other words, that the vessels of the uterus do not inosculate with those of the funis umbilicalis; and consequently that no fluid can pass directly from the one set of vessels into the other. But although it be certain, from this anatomical fact, that the blood cannot flow in a continuous current from the arteries of the womb, into the umbilical veins of the placenta, still it is maintained by many of the most celebrated physiologists, that the placental extremities of these vessels possess the power of absorbing the fluid which is brought in contact with their mouths. It is thus, as is now very generally believed, that the fœtus draws its nourishment from the mother; not, indeed, by receiving through these vessels the maternal blood, but

by the absorption of a chylous fluid, which is secreted and poured into the cells of the placenta, by the arteries of the uterus. The only difference existing between this doctrine and the one advocated by Dr. C., consists in the former alleging, that the umbilical veins possess the power of absorbing this chyliform fluid; and the latter denying this function to these vessels, and maintaining that it is performed by a set of lymphatics, which are supposed to exist in the *funis* and placenta.

Let us now examine some of the arguments with which our author attempts to disprove the power of the umbilical veins to absorb the fluid deposited in the cells of the placenta, and which, it is now pretty generally believed, is destined for the nourishment of the fœtus.

All the arguments which he brings forward, upon this head, are taken from Monro's essay on fœtal nourishment, and go to show that *there is no transmission of blood from the mother to the child.* This point, we conceive, has been ably and conclusively handled by the author just mentioned. We cannot however see, how these same arguments can prevail against the opinion that the umbilical veins perform the office of absorbents, and that *they* carry the nutritious fluid from the placenta to the fœtus. Dr. Chapman, it is true, says, "that the arteries of the cord have no openings," yet this is contradicted by able anatomists.* "The placenta," says Monro, "is covered on the side next the womb with a fine membranous continuation of the chorion. The extremities of the umbilical vessels pierce this membrane, and show their very small orifices on its side next the womb." Rauhault calculates that only one seventh part of the capillary branches of the human umbilical vessels reach the exterior surface of the placenta.

"The effusion of blood by one set of vessels," says Dr. C. "and absorption by the other is a creation of the imagination, and has no real existence." In support of this assertion he does not advance a single direct argument. For those which follow, go only to prove that there is no immediate continuous vascular connection between the parent and the fœtus. They do not dis-

* Medical Essays and Observations, vol. ii. p. 128. Ruysch. Thes. 5. n. 41. Rauhault Memoires de l' Acad. des Sciences, 1714 and 1716.

prove the *absorption* of blood by the umbilical veins. "It is well known," says Dr. C. "that after the expulsion of the child, and the cord is cut asunder, no hæmorrhage ensues, on the side of the mother." Now, this proves that there is no continuous course of vessels from the mother to the child; but it does not prove that there may not be a sanguineous absorption by the extremities of the umbilical vessels, during the period of utero-gestation. As the placenta and cord derive their *life* from the child, it is evident, that when they are separated from it, they must at once become a *lifeless mass.* But absorption is a *vital* process. The vessels, whatever they be, that perform this function do so by virtue of their vitality; deprive them of this, and you deprive them of their power of absorption. No mechanical pressure can supply the loss of vitality in the absorbent.* Hence this argument does not disprove the absorption of blood by the umbilical veins; it merely shews what might be expected, that a *dead* vessel does not absorb, and consequently that in the instance given, (granting that the blood thus circulated from the mother to the fœtus) hæmorrhage could not possibly have taken place.

The same objection applies to the following experiment, performed, as it is alleged, by our author:

"The side of a pregnant bitch was laid open and the umbilical vein of the fœtal pups divided. As I anticipated the hæmorrhage was profuse, and the fœtus, on inspection, nearly exhausted of blood. But in repeating the experiment I previously tied the artery of the cord, and little or no loss of blood was sustained."

This experiment, which is given without the name of any other person in testimony of its having been regularly performed, (a courtesy, which physiological experimentalists have always thought proper to observe towards their readers,) may serve to show that there is no immediate and continuous vascular connection between the mother and the child, but it does *not* prove that the maternal blood, after having been poured into the placental cells, is not absorbed by the extremities of the umbilical vein. For, as soon as the funis of the pups was divided, the placenta became a lifeless body, and its vessels were therefore incapable of absorb-

* Monro on Fœtal Nourishment.

ing, which, as we have just observed, is a vital process. There was no necessity of performing this cruel experiment to disprove the existence of a direct vascular connection between the mother and the fœtus; for the injections of Monro, have long ago settled this subject beyond all doubt.

We ourselves do not believe that there is any *blood* transmitted from the mother to the child, either by a direct communication or by absorption. We object to these arguments merely on the score of their not proving what the author alleges them to do.

In another experiment of our very philosophic author, he says, "he fed different animals, while pregnant, on madder, and though *several* parts of the maternal system *was* (were) manifestly coloured, I never could detect the slightest appearance of it, either in the fœtus itself, the *liquor amnii, or any portion of the secundines.*"

What a specimen this of physiological wisdom! Dr. Chapman gravely tells us, that after feeding pregnant animals on madder, *neither the secundines* nor the liquor amnii were found to be imbued with the peculiar colour of this substance!! We are not at all astonished at such information, for we are aware of the fact, and Dr. Chapman ought not to be ignorant of it, that *madder* when taken into the system, imparts its colour *to the bones only*, and can never be detected in any other part of the system. Dr. Gibson,* of Manchester, in a very philosophical inquiry into the effects of madder on the animal system, has proved incontestibly that no parts of an animal *except those which contain phosphate of lime*, can become tinged with the colouring particles of madder. What then, we would ask Dr. Chapman, "*were the several parts of the maternal system manifestly* coloured." We beg the Doctor to be a little more precise when he next informs us of his physiological experiments. To us it is clear that the Doctor's eyes must have been deeply jaundiced, or *that he never performed this experiment at all*, since he says that he saw *several* parts of the maternal system *manifestly coloured;* a thing which *was utterly impossible! Nimium ne crede colori.*†

* Memoirs of the Literary and Philosophical Society of Manchester—Second Series, Vol. I. p. 146.

† The Editor of the National Gazette, speaking of this paper, says—"The

Speaking of the sympathy which exists between the uterus and the mammæ, Dr. Chapman says: "these organs often exchange functions. Thus in a state of pregnancy the office of forming milk, or at least a fluid closely resembling it, as nourishment of the fœtus, devolves on the uterus; but as soon as the child is born, this duty is transferred to the mammæ, the uterus relapses into a state of inactivity, for a season; *and the epigastric artery, the immediate instrument by which the secretion is accomplished*, becomes once more small in comparison with its preceding size."

Had we been told by any one that Dr. Chapman had asserted in his lectures, "*that the epigastric artery was the immediate instrument by which the secretion in the uterus is accomplished,*" we should have regarded it as a gross attempt to slander his reputation; for we could not have believed that any regularly educated physician, much less one who occupies the responsible station of a professorship in one of our first universities, could be so very ignorant of the structure of the human body, as to be capable of uttering such egregious nonsense. Notwithstanding this, it is a fact, that Dr. N. Chapman, Professor of the Institutes and Practice of Medicine in the University of Pennsylvania, does state, in the article under review, that the "epigastric artery is the *immediate instrument*," of a secretion performed in the uterus! As ignorance, however, is no crime, we could readily pardon such an absurdity even in a professor; but in referring to Blumenbach as authority for such nonsense, we do know, that he has been guilty of a slanderous outrage upon the scientific reputation of that illustrious German physiologist. If Dr. Chapman will question any of his "second course" students, upon this point, he will learn from them, that the epigastric artery has no more to do with the uterus, than it has with the great toe; that it does not even arise from the

positions taken are sustained by several well contrived experiments, and by a vast number of facts and observations; and, upon the whole, the writer may be considered as having placed his physiological views on this subject, upon an incontrovertible basis." The above are some of the writer's "well contrived experiments," and the last is one of those "facts" which place "*his* physiological views on this subject upon an incontrovertible basis!" We cannot help reminding the intelligent editor of the good old maxim—*ne sutor ultra crepidam.*"

same trunk that gives origin to the uterine arteries, and consequently, that it neither receives nor transmits a drop of blood to that organ! Dr. Chapman evidently regards this connection between the epigastric and mammary arteries, as one of his *triumphant* arguments. He says: "when the milk does not come at the ordinary period after delivery, the lochia are much more abundant, and sometimes are *not a little* changed in colour and consistence, by the admixture of a lactescent fluid. *These facts I consider as irresistible.*" We are not at all surprised that Dr. Chapman, who calls *the epigastric artery the immediate instrument, by which the uterus secretes its lactescent fluid*, should consider "these facts as irresistible." But we, who put no faith in this "*immediate instrument*," see nothing so "*irresistibly*" demonstrated, by "these facts," as his ignorance of the anatomy of the parts in question, and the absurdity of his conclusions.

Our own opinion upon the subject is, that the *albumoid* liquor, which Harvey, Haller, Blumenbach, and others have shown to be secreted into the cells of the placenta, is taken up by the small orifices of the capillaries of the umbilical vein that open on the surface of the placenta and its cells, and carried by this vessel to the fœtus, which by its own organic powers converts it into blood. There is perhaps no blood returned from the fœtus to the mother, and the quantity of nourishment required for the support and growth of the fœtus being but small, the absorption must be carried on very slowly. The facts and arguments first brought forward by Monro, prove incontestibly, that there is no interchange of blood between the fœtus and the mother. The fœtus receives, according to that celebrated writer, not red blood, but a chyliform fluid, which serves for its nourishment, and which it converts into blood, like the chick in ovo. The recrementitious part of the fœtal blood, is, according to him, emptied into the sinuses of the womb to be carried away like the bile, pancreatic juice and other liquors of the adult poured into the intestines to be removed from the system.

Dr. Monro does not seem to have made up his mind whether the absorption of this nutritious fluid is performed by the extremities of the umbilical vein or by a set of absorbent vessels.

If the former he said should be denied, he would explain this absorption, in the only possible way left, *i. e. by supposing the existence of lymphatic absorbents in the funis.* In this latter opinion Dr. C. agrees *in toto* with Dr. Monro; and as this is the essential point of the doctrine so adroitly defended by our ingenious author, we think it would have been decorous in him to have quoted the Edinburgh professor. "The fœtus," says Dr. Chapman, must receive its nourishment through the blood-vessels of the cord or by absorbents: there is no other way in which it can be conveyed. Having shown that it is not by the former, *am I not entitled to infer that it is by the latter?*" Certainly sir, and so are *we*, with just as great claims to originality as you, since both the arguments, and the conclusion, are neither yours nor ours, but those of Alexander Monro of Edinburgh, who lived in the beginning of the last century.

In thus taking notice of this favourite production of our author, in a way which may, perhaps, be deemed rather uncourteous by some of our readers, we declare that a sense of our duty alone has led us to the disagreeable task. We take no delight in inflicting castigations of this kind, upon offenders, however glaring the fault, or contemptible the imposition. The present instance, however, is one which is peculiarly calculated to attract our attention. The lofty standing which the author of this article has taken, not only points him out a legitimate object of a rigorous scrutiny, but lays him open to animadversions, which applied to men of humbler pretensions, might be justly deemed as needlessly severe. Standing forth, as he does, in the proud and daring attitude of defiance; and offering himself in the true spirit of chivalrous patriotism, as the champion of the insulted genius of our country, we have a right to await from him, a noble and honourable conduct in relation to his own literary and scientific career, and a reputation unstained with the low and contemptible folly of palpable plagiarism. In our capacity of editors, we too, hold ourselves bound to vindicate the honour and reputation of our native science. We regard it, as one of our first duties, to watch over the interests of medical science, and to oppose error and fraud wherever they be found. It is a just sense of these important duties, that has induced us to

speak thus plainly to the author of the present article. We considered ourselves bound, if we meant to perform our functions with honesty, to notice the gross errors, the absurdities and contradictions, of this favourite production of our author; and above all, to expose the more censurable and shameful attempt which he has here made to appropriate to himself the sentiments and doctrines of others. Should the Journal which we are now reviewing, find its way into Europe, what will our transatlantic brethren say, when they see the lofty pretensions avowed in its prospectus, and insinuated in its motto, supported by such a specimen of literary integrity and scientific research? What will they think, when they learn, that the editor of this Journal, the avowed object of which is to vindicate American science, advances as his own, a doctrine which has been well known in Europe for nearly a century? What, we say, when they hear him assert, that the epigastric artery is the "*immediate instrument*" of a secretion performed in the uterus? And what, when they find him gravely declaring, that he saw the *colour of the madder* in *different parts* of a pregnant animal that had been fed on it, and that he looked for it in the *secundines* of the fœtus? When they notice these things, with many more equally preposterous, and then turn to the disgraceful expedients which he has used to substantiate his claims to originality, they will, we greatly fear, entertain but very little reverence for our native science. Indeed when we reflect upon the effects which such graceless pretensions must have on the literary and scientific reputation of our country, and more especially when put forth by the *dii majores* of our own science, under the assumed office of its champions against foreign aspersions, we feel but little disposed to show any quarters to the inglorious delinquencies of our author. Will not our transatlantic enemies appeal to this *morceau* of one of "our most distinguished physicians," and with a malicious, sneering triumph, exhibit it to the world, as evidence, confirmatory of their foulest slanders and bitterest reproaches? We fear, that even the powerful author of the *Appeal* will not be able to remove stains so deeply and glaringly infixed on the annals of our native science. Surely we do not stand in need of such aids, "to evolve and stimulate the genius of our

country to invigorated efforts," or such defenders against "foreign usurpations and calumnies."

> Non tali auxilio, non defensoribus istis,
> Tempus eget.

"*Art. II. The Double Canula and a Wire recommended in the operation of extirpating Scirrhous Tonsils, and Hemorrhoidal Tumours.*—By PHILIP SYNG PHYSICK, M. D."

Although the paper communicated by the professor of anatomy of Pennsylvania, is by no means a very important one; although the style in which it is conveyed is most defective, still with joy we hail its appearance: we flatter ourselves that it is merely a pledge and promise of other and richer offerings which we shall soon receive from its justly celebrated author. We confess that we have felt inclined to blame Dr. Physick for his long silence. A man placed in the high situation which he has occupied in his profession, having an extended field for practical observation, and a mind well suited for drawing just, and philosophical conclusions from the facts which are placed under his consideration, owes to his profession a duty which cannot be discharged by the publication of a few *trifles*. We are aware, however, that Dr. P. has been peculiarly circumstanced.—The various, and laborious duties of his practice removing in some measure from him the ability to collect and arrange his observations, and the belief that these would be sufficiently disseminated through the medium of his lectures, taking from his mind, the positive call to circulate them by means of the press. The question "*What does the world yet owe to American physicans and surgeons?*" has been most arrogantly demanded. It is not our duty to content ourselves with the answer, "*Emphatically, we have done our duty.*" Nor are we to elevate the character of the medical literature of our country by the publication of "The MEDICAL CLASSICS." To Dr. Physick we anxiously look, and as he will now in a great measure have the toils of lecturing removed from himself to his adjunct, Dr.

Horner, we should fondly anticipate from his pen some national work on surgery, so rich in facts and observations as to testify not only to the Edinburgh Critics, but to the whole world, that America is not behind the European nations in the extent and value of her chirurgical knowledge.

As we are most anxious, for the reasons above detailed, that Dr. Physick should come more fully and more frequently before the public, we shall not be hypercritical in noticing his little essay.

The practice of removing portions of the tongue, the tonsils, hemorrhoidal tumours, &c. &c. by ligature is one which has been long and extensively introduced into the practice of surgery. It has been *generally* customary that the wire or thread which was carried around the part to be removed should, after it had been tightened so as to prevent its circulation and destroy its vitality, be suffered to remain until the portion of substance which it enclosed had sloughed off. In those cases of enlarged tonsils or hemorrhoidal tumours where the wire and double silver canula were employed, the canula being allowed to project from the mouth or anus until the separation occurred was not only attended with very considerable inconvenience, but, aggravated very much the sufferings of the patient. Experience and observation has taught that after the vitality of a part is, from the obstruction of its circulation, or any other cause completely destroyed, life can never again be restored to it, and that it must therefore be thrown off. Dr. Physick proposes that so soon as we have, by the pressure of the wire, completely destroyed the life of the part we are desirous to destroy, that the wire may then be loosened and removed, and that the separation will be as perfect as in those cases where it is permitted to remain. The method of performing the operation, and its effects, will be understood from the following extract:

"The patient is to be seated opposite a window, and his tongue must be held down by an assistant, with the handle of a large spoon, or with a spatula. The surgeon is then to slip the noose over the tonsil, and down to its base, taking care not to include the uvula, which, when the swelling is large, is apt to be in the

way. The wire is then to be drawn sufficiently to fix it loosely on the part, and the surgeon is to satisfy himself, by an attentive inspection, that it is properly applied. This being accomplished, the wire is to be taken hold of with the pliers, and drawn through one side of the canula, so as secure it at once on the base of the tonsil as firmly as possible, and then to fasten it on the arm of the instrument, and thereby prevent all entrance of fresh blood into the tumour. This method of stopping the circulation of blood in the swelling necessarily occasions severe pain at the moment; but the severity of it soon ceases.

"On examining the tonsil after a few minutes, its colour will be observed to be changed to a deep purple or almost black, and its surface smooth and polished, owing to the exterior membrane being stretched.

"It has hitherto been my custom to allow the instrument to remain thus applied for twenty-four hours, with the view of destroying the life of the enlarged gland. I am, however, of opinion, that a much shorter time would be sufficient, as eight or twelve hours, which I propose soon to ascertain. After having destroyed the life of the swelling by the above means, the next step of the operation is the removal of the instrument, which is very easily accomplished, in the following manner. Take a firm hold of the end of the canula projecting from the mouth, then disengage the wire on one side from the arm of the instrument; straighten it, and with the pliers push a small portion of it back through the canula, and repeat this until the noose is so much enlarged as to slip off the tonsil.

"The operation is now completed; the tumour appears shrivelled and of a dull white colour; the patient suffers no pain; the inflammation is moderate, and, after a few days, the dead parts are separated and thrown off, either entire or in fragments, which are sometimes spit out, sometimes swallowed. Until the separation is completed the breath is somewhat offensive. I have never had any trouble with the small ulcer remaining after the separation of the tumour. It has healed so rapidly as generally to escape notice."

"*Art. IV.—Cursory Remarks in relation to the objects of the Materia Medica and Pharmacy, &c. By* JOHN R. COXE, M. D."

This article is devoted to an exposition of "*the great evils of an extensive Materia Medica.*" Divers very cogent reasons are given in support of this opinion; such as, 1. "No physician, however extensive his practice, does actually employ more than fifty *different* and *distinct* articles. 2d. When the list of articles is very extensive," it renders us incapable of duly estimating the real value and medicinal properties of each remedy in particular. 3d. Many articles are introduced on the authority of "practitioners or others, who frequently are ignorant of the nature and properties of even those of established use." After these and a few other arguments of minor value have been exhibited in a variety of phases, our author introduces a multitude of solid observations upon the topic he discusses. He tells us for instance, "that for many of our most valuable articles, we have as yet found no substitute amongst ourselves!" That "antimony and mercury still reach us from a foreign source!" "That *if* we can domesticate any of the useful remedies of foreign origin, reason will justify, and success will *probably* follow the attempt!" That if very efficacious remedies are "to be procured within the limits of our own country, let us cheerfully admit them into our list."

These very edifying observations are delightfully interspersed with a most overwhelming variety of truisms and every-day observations. The author very considerately, we think, does not confound his readers with reflections drawn from the deep well of philosophy. He has picked up his ideas at the top, and seems moreover to have culled the simplest he could find. This is very prudent:

> Ludere qui nescit, campestribus abstinet armis;
> Indoctusque pilæ, discive, trochive, quiescit,
> Ne spissæ risum tollant impunè coronæ:

Our author says, "the indications to be answered in the practice of physic are few in number; nor less so are the means by which such indications may be fulfilled."* This may all be true, and

* *Cathartics*, emetics, diaphoretics, sialagogues, expectorants, antilithics, emmenagogues, tonics, stimulants, are mentioned by our author.

yet not militate against a copious pharmacopœia. Surely Dr. Coxe cannot believe that all the purposes of the healing art can be answered by the fulfilment of the indications we possess. There are a host of maladies for which we possess invaluable remedies whose action cannot be referred to the fulfilment of any obvious indication. When we exhibit colchicum in gout, under what indication is it given? When arsenic cures venereal nodes, what indication is answered by it? The fact is, our knowledge of the efficacy of medicines in a variety of diseases rests, as yet, wholly upon experience: we prescribe them often, not because we see certain indications to be answerd, but because experience has made us acquainted with their usefulness. Medicine has not yet advanced to that degree of perfection, when the treatment of diseases shall be conducted on purely scientific principles.

We will conclude these remarks by the following quotation from Dr. Chapman's therapeutics, to the correctness of which we fully assent. "My impression is," says the Dr. "that scarcely any two agents produce entirely the same effects;* and hence the infinitely diversified shades of diseases, and the necessity for a variety of remedies in the management of them."

"*Art. V. Remarks on Bronchocele or Goitre. By* W. Gibson, M. D."

We have, after very considerable exhaustion, got through the remarks on Bronchocele, or Hernia bronchialis, or Gongrona, or Hernia gutturis, or tumidum Guttur, or Derbyshire neck, or monstrous craw, or Gheig, or Aubi, or Bà, or Ke-ba, or the disease of the thyroid gland, for which the *very profound* and *learned* Professor of Surgery in the University of Pennsylvania has, with the most unwearied industry, hunted out the above, and we dont know how many more monstrous appellations.

The *learned* author has obligingly informed us, that the bronchocele is common in the neighbourhood of Berne, of Friburg,

* See Wilson's strictures on the Brunonian Doctrine of Life, &c.

Lucerne, Aigle, Bex, Dresden, in the vallies of Piedmont and Savoy, in most parts of Vallais in the Valentine, in La Batia, Villeneuve d'Aoste, Martigny, St. Maurice, St. Jean, St. Michel, Aiguebelle, at Cevennes, Soissonias, Vosges, Rouergue, Doubs, Ardeches, Derbyshire, Buckinghamshire, Surry, and Norfolk. At Boutan, Morung, Nipal, Almora, Bishee, Gooch, Bahar, Rungpore, Dinagepore, Purnea, Tirrooto, Betiah, Gooracpore, Barraitch, Pillibeat, Rochilcund, Hurdewar, Bambarra, Sumatra, Jacaltenango, Scapula, the hernia bronchialis prevails. In Santa Fee, Guatemala, Nueva, Gallicia, and Nicaragua, Hunda, Monpar, and on the borders of the Magdelaine river, the ba, or the ke-ba is common. The ferrymen at Carthagena are not subject to it, "but many persons at the Isthmus of Darien are horribly disfigured by enormous" aubis. Further, the author has kindly mentioned to us, that Derbyshire neck is met with in North America, and as he has taken the trouble to inform us particularly of the different situations where in our own country the disease is found endemical, we shall copy his list for the *instruction* of our readers. Hernia gutturis is very common at Detroit, Lake Ontario, Oneida, Erie, Huron, and among the Tuscarora, Seneca, Oneida, and Brothertown Indians. It is found at Bennington, Chittenden, Camden, Sandgate, Chester, Old Fort, Schuyler, Oneida Village, German Flats, Fort Herkimer, Fort Dayton, Henderson town, Onondago valley, Canasaraga, and Brothertown; and Dr. Gibson is informed by "*Philip Church, Esq. who resides at Angelica, in Alleghany county, state of New York, that goitre is a very frequent complaint in his neighbourhood and the surrounding country.*" The Gheig is a very common disease in Pittsburg, on the waters of the Alleghany, Sandusky, Monongahela, French Creek, at Cannonsburg, Brownsville, and throughout the county of Somerset the gongrona is met with. The tumidum guttur is found in some parts of Virginia, especially at Morgantown and on the banks of the Cheat river. In certain situations on the western shore of Maryland, and in North and South Carolina, "the monstrous craw" is occasionally met with.

We must honestly confess, that when we read the introduction to the remarks on bronchocele, we were at a loss to understand

why the author had taken so much trouble to furnish us with so many names for this disease. But now that we have come to write a review of his observations, we have become satisfied that he has judged much more wisely what was for our interest, than we did ourselves. Our readers will remark, that although we have condensed into a small space nearly all the information which will be found in the Professor's essay, that still our author has furnished us so bountifully with titles for the disease which he describes, that we have never had occasion to injure the *beauty* and *softness* of our sentences by repeating the same term twice.

It may perhaps, be expected by the inconsiderate, that after they have taken the trouble to spell over the names of all the situations where *bà* is found endemical, that they will be repaid for their toil by an interesting series of speculations, as to the probable causes which render the individuals of certain districts of country more obnoxious to the complaint than others of their countrymen: but as Adam Smith has ably demonstrated that a division of labour is the soul of manufactures, our author has, we suppose, thought that the manufacturing system may with advantage be applied to the collecting of materials for the Philadelphia Journal; and as a little piece requires to go through all the hands belonging to the workshop before it is completed, it may be the editor's intention to bring the "monstrous craw" through all his professional heads, before it is to be looked on as an understood or elucidated subject. If we are correct in our conjecture, we would congratulate the editor on the wisdom he has shown in selecting Dr. Gibson for the duty he has imposed upon him, for we are satisfied, that he could not have pitched upon any other man in the country, who was so well suited, both "morally and physically," for the execution of this labour. It may, perhaps, be presumptuous in us to give the learned editor a hint, but, as we would really wish to see the subject finished in the best possible style, we would humbly recommend that he keep the fictitious part in his own hands.

Although it be evident that the principal object of Dr. Gibson's paper is to astonish, not instruct his readers, still there is the claim

to a discovery most cunningly introduced. A young lady had consulted the Doctor, "who had laboured for two or three years under goitre." He attended her for several months, and "employed every remedy likely to prove of service, but in vain." The Professor finding every thing useless, and growing tired of importunity, gave his fair patient a box of cicuta pills, "which had been prepared for another purpose." There is a vulgar adage which we might introduce here, but we shall content ourselves by saying, that Dr. Gibson was fortunate, as the pills had the effect of removing the *bà* from the young lady's neck in three weeks. The lady having been cured by having from her importunities induced her physician to prescribe for her *par hazard*, has no title to complain. But we hope our readers will not be led, from the good luck of Dr. Gibson, to answer the importunities of their fair patients by thrusting into their hands "sans consideration," a box of pills.

We allow that the discovery is very modestly claimed; the author, after talking fully on its importance, concludes, "it is possible that cicuta may have been employed before in the treatment of this disease, but I have not been able to meet with any mention of it." We can tell the author, that it is not only possible, but that it is most certain, that cicuta has been used before he gave it to the importunate young lady. It is, in fact, one of the commonest medicines employed in the cure of bronchocele, and had our author not examined the works which he has quoted, merely for the purpose of appearing learned, he would have found, that it was very generally recommended. His "*favourite author*," Mr. Samuel Cooper, mentions it.

Although we have laughed at the absurd affectation of learning which Dr. Gibson has shown in his essay, we consider it our duty to tell him seriously, in conclusion, that his conduct is deserving of a severer criticism. The dispensation of the philosophy of Newton has removed from science all that darkness which its first followers had thrown around it, for the purpose of astonishing and confounding the vulgar. It is now the duty of men of education to promote, by their learning, the elucidation of facts; not to introduce it for the purpose of confounding and bewildering those they

are anxious to instruct. But this we need hardly mention, for we are of opinion, that no man who is really learned, will write, as Dr. Gibson has done: the fact, that "a little learning is a dangerous thing," should never be forgotten, and he whose conscience whispers that he has nothing more, will, if he attempt to write profoundly, only sink into a *bathos* so dark and unfathomable as to be equally unintelligible to others as it is to himself.

"*Art. VI. An Essay on the Rupture on the Uterus.* By WILLIAM P. DEWEES, M. D."

Although containing nothing new, this is an excellent article. It affords, we conceive, a very creditable specimen of the author's habit of close observation, as well as of his good sense, and the sound practical character of his mind."

Dr. D. thinks, there is just cause to believe that the uterus is much more frequently ruptured, during parturition, than what is commonly supposed. "Many of the sudden deaths, during labour or quickly after, are entirely unaccounted for, though the symptoms attending the illness justifies the belief, that they were from this cause."

As it has been very generally held, by writers of the greatest celebrity, that nothing can be done, or ought to be attempted, for the relief of a female, who has suffered this dreadful accident; our author, in the first place, enters into an elaborate and we think successful inquiry concerning the propriety of this abandonment of the unfortunate sufferer.* Dr. Hunter considered it as an

* It may be observed that this opinion is no where so prevalent as in England. On the Continent, and particularly among the Germans, the practice of performing gastrotomy where the child has escaped into the cavity of the abdomen, from rupture of the uterus, is very generally recommended. Bernstein† says: "If therefore the child cannot be drawn back after having partly escaped from the uterus, or where it is perfectly evident that it has passed entirely into the cavity of the abdomen, and there is no possibility of delivering it by the natural passages, and the mother is not two much

† Praktishes Handbuch der Geburtshülfe. 4. Band. 86. Frankenthal, 1791.

act of wanton and hopeless cruelty, to attempt to deliver a patient suffering under this accident. Dr. Douglass* was the first, says our author, who seriously called the propriety of this opinion in question. He delivered a patient, whose uterus had ruptured, and was completely successful. Our author very justly expresses surprise at the following aphorism of Dr. Denman: "*When the uterus is ruptured at the time of labour both reason and experience show that the patient has a better chance of recovering, by resigning the case to the natural efforts of the constitution, than by any operation or interposition of art.*" This opinion is contrary to "reason and experience;" and is in opposition to the testimony of the highest authorities. In proof of this Dr. Dewees relates instances "from authors of deserved faith" where the patient was recovered from this deplorable accident, by the interposition of art. The first instance is quoted from Heister.† "In this case the intestines were distinctly felt through the rupture of the uterus and through which the fœtus was extracted." The woman recovered. Another case is given from Dr. Douglas.‡ One from Dr. Hamilton.§ Dr. Ross‖ also relates the case of a woman who suffered this accident in two consecutive labours, and yet recovered. A case is also quoted from Mr. Kite.¶ In a copy of MS. lectures of Dr. J. Hamilton, the present Professor of Midwifery of Edinburgh, there are two cases related, one of which he himself attended and says it was "one in which almost every circumstance was unfavourable;" for "in bringing the child through the lacerated part he felt the uterus tearing more; the woman lost three pounds of blood, yet she recovered and afterwards had children. The other case was

exhausted, the operation of gastrotomy should immediately be resorted to, as the only possible means of saving either the mother or the child. This operation is much more easy to perform than the Cæsarean section, *and does not appear to be more dangerous.*"

* Essay on rupture of the uterus, p. 7. 1784.

† Institute de Chir. tom 2. p. 137.

‡ Essay on rupture of the uterus, p. 7.

§ Outlines, p. 344.

‖ Annals of Medicine, vol. iii. p. 377.

¶ Mem. Med. Soc. of Lond. vol. iv. p. 253. Annuaire Medico. Chirug. tom. i. p. 542.

produced by a fall from a cart. The Cæsarean operation was performed and the woman recovered. Mr. Thibault* relates a similar case; gastrotomy was performed with complete success. Baudelocque† relates a case where gastrotomy was performed with complete success. Mr. Hugo‡ also relates a successful case. In all these cases the woman escaped, but the child was dead. This says Dr. Dewees "was rather owing to the time at which art interfered, than to the mode adopted." Dr. Dewees, after having adduced these cases of recovery from lacerated uterus, when assisted by art, challenges the advocates of Dr. Denman's opinion "to furnish a single instance of recovery from a ruptured uterus or vagina, at full time," where the fœtus was suffered to remain undelivered.

Dr. Dewees is of opinion that the substance of the uterus may be lacerated independently of its peritoneal covering, and that the fœtus in passing through such a rupture in the uterus, will push before it the peritoneum, and be thus prevented from passing directly into the cavity of the abdomen. It is in this way that he explains the cases that have been related by authors of recovery from lacerated uterus, where no assistance was affored by art. In such cases the child is lodged in a sac formed by the peritoneum, and has no communication whatever with the internal cavity of the abdomen. Our author's observations on this head are reasonable, and much to the point.

After having remarked upon the uncertainty of any sign as a forerunner of this accident, the author proceeds to enumerate those symptoms that generally attend the moment rupture of the uterus has taken place. "For the most part the woman feels an acute pain at the part where the rent has happened, she generally shrieks out, and declares that something unusual has happened to her; the rupture is sometimes accompanied with a noise that is audible to the by-standers; a discharge of blood of greater or less extent is noticed from the vagina; her face becomes pale; her respiration is hurried; she becomes sick at the stomach, and most

* Journal de Med. for 1768.

† Heath's Translations, vol. iii. p. 430.

‡ Med. and Phys. Journal for March 1808.

frequently vomits; the matter discharged is sometimes only the common contents of the stomach, at other times very dark coloured, and even black; the pulse becomes extremely frequent, small, fluttering or extinct; the woman complains of a mist before her eyes, loss of sight, and extreme faintness; a cold clammy sweat bedews the whole body, and convulsions, and death follows if she be not speedily relieved."

Several circumstances seem to modify these symptoms.

1. When the rupture happens in the neck or body of the uterus the pains almost always cease or become very weak, having no propulsive effects. The woman generally rapidly sinks into a state of extreme weakness "either in consequence of the importance of this organ" or the profuse hæmorrhage.

2. When the child has escaped wholly into the cavity of the abdomen; "*pain instantly ceases*, and the most distressing and alarming symptoms are almost certain to follow." If the child is not entirely protruded, "the pains may continue and even effect the delivery."

3. If the peritoneal covering of the uterus is not ruptured but merely pushed forward into the shape of a sac by the protruding child, the symptoms are generally much milder and the woman's recovery is much more probable.

However decided the character of the symptoms of ruptured uterus may be, they cannot be relied on exclusively; a certain diagnosis can only be had by a careful examination, per vaginam, and of the abdomen externally. If the fœtus be in the cavity of the abdomen, it may be detected by its motions if alive, or by tracing with the hand its limbs through the thin sides of the abdomen. In this case the contracted uterus will generally be found occupying its usual place; should we be led to suspect a rupture of the uterus, we have a right to conclude our suspicions ill-founded, or that the child is still in the uterus, notwithstanding the existence of a rupture, if this organ maintain its globular form. "If the accident happen after the rupture of the membrane, the presenting part will either recede beyond the reach of the finger, or will be so easily forced back as immediately to excite alarm, if not confirm suspicion." To be certain, under such circum-

stances, the hand should be introduced into the vagina and a careful examination made. If the uterus itself be ruptured, the hand may readily be passed into it, if the labour be already considerably advanced, and thus a knowledge of the exact situation of the patient be obtained. If the os tincæ be as yet not sufficiently dilated, it would be very wrong to use any force to dilate it. We may, however, introduce a finger, and by this means ascertain "whether the fœtus be entirely in the uterus, for in this case it will be very near the os uteri; if it has partially escaped into the abdomen, it will most probably be more remote; and if entirely in that cavity, it will no longer be within the reach of the finger.

There does not exist any difficulty in ascertaining the precise nature of this accident when it happens in the neck of the uterus; for when the laceration takes place in the neck, the wound for the most part is extensive, and permits the hand to pass without difficulty into the cavity of the abdomen.

In the last place, Dr. Dewees considers the "mode of proceeding under all the various circumstances with which this accident may be complicated." He places no reliance in the powers of nature to effect a cure, when the child has escaped into the cavity of the abdomen; and is of opinion "that nothing but the prompt and judicious interference of art, can rescue the unfortunate woman from the impending fate with which a ruptured uterus threatens her." In order to afford any *probable* chance of success by the interference of art, "it must not be delayed a moment beyond the detection of the accident."

In the treatment of this afflicting accident we are restricted to the three following general modes:

"1. To attempt delivery per vias naturales.

2. To attempt it by gastrotomy, or the Cæsarean operation.

3. To leave the case to nature."

The first mode is not always proper, nor even practicable. We are therefore obliged to employ the second mode for relieving our patient, if we wish to make any rational effort to save her. Upon the subject of gastrotomy, Dr. D. says: "Gastrotomy and the Cæsarean section present horrors to the mind peculiarly their own:

nor should we be able to overcome the appalling sensations they produce, if we were not influenced by paramount considerations. To save life is a strong motive to the operation; and to be snatched from death is a powerful inducement to submit to it." "It has been called a '*horrible expedient*' by Dr. Douglass. It is so confessedly; but this is not to be the test. Its utility alone ought to determine, whether it should be considered as a resource of our art, or be for ever proscribed. That it has been successfully employed we are not at liberty to doubt; nor is there any testimony that it has been either wantonly employed, or that it has added new sufferings or new dangers to the already almost certainly fatal disease, for which it is proposed as a remedy." The author then brings forward cases in testimony of the successful performance of this operation. "Mons. Thibaud des Bois, a surgeon of Mans, has given an account of this operation having been successfully performed on a woman several hours after the accident, though too late to save the child." M Laussus quotes a history of this operation having been twice perfomed, with entire success, in the same woman.

To us it appears clear that our author has made out his point by a body of incontestible testimony as well as by sound and legitimate deduction. The operation which he advocates, though appalling in the extreme, holds out a remedy, which promises at least some hopes of success, in a disease which without such aid is certainly fatal.

The following case of rupture of the uterus, in which gastratomy was successfully performed, does not appear to have fallen under the notice of Dr. Dewees. On the first of September 1817, Mary Debout, aged 33 years, of weak constitution, having arrived at the full term of her fourth pregnancy, was taken with labour pains. An obliquity of the child's head on one side of the superior aperture of the pelvis, offered some resistance to the expulsive efforts of the uterus, enfeebled, probably by a fall which the woman experienced a fortnight previously. The womb gave way by a transverse rent across its fundus, and the child escaped into the cavity of the abdomen, where it became prominent while the uterine tumour disappeared. Gas-

trotomy was the only resource. Twelve hours elapsed before it was performed. An incision was made in the linea alba, beginning two inches below the navel, and continuing it to within two inches of the pelvis. The child was dead, and a considerable quantity of blood was found extravasated into the abdomen, mixed with liquor amnii. The intestines were inflamed, and in some places discoloured by the long pressure of the fœtus. The extravasated fluids were removed and the lips of the wound brought in contact and kept by sutures. Fomentations, antiseptic drinks, enemata and even cordials, were given, without any depletion apparently, and the patient recovered in two months.*

"*Art. VII. Cases illustrating the use of the Tourniquet in Palsy. By* S. CALHOUN, *one of the physicians of the Pennsylvania Hospital.*"

The author begins by stating that, "the restoration of the power of the muscles, debilitated by long inactivity, is an *important* indication in the treatment of *palsy!*" This is a very acceptable piece of information, since it might have been forgotten, that it is at all important, to restore the power of the muscles in palsy!

The first case related in illustration of the efficacy of the tourniquet in effecting this '*important* indication,' is that of a man who after having slept in a sitting posture for four hours, awoke "with the usual symptoms produced by pressure upon the sciatic nerve." The torpor gradually left the thigh and leg, but the extensor muscles of the foot continued paralytic. The foot stood out at right angles from the leg, and imparted the sensation of a dead weight appended to the extremity. The extensors of the foot were wholly paralytic; the skin around the ankle joint was insensible "to the prick of a pin, or to pressure by the finger; the sensation of heat remained perfect." "The conjecture that the quantity of blood in the paralytic parts was deficient, induced the application of the

* Johnson's Medico Chirurgical Journal, June 1820, page 108. See also Journal Complement, December 1819.

tourniquet, as an increase of circulation had been observed to follow its removal." The tourniquet was applied to the thigh half an hour repeated four times a day, so that the artery upon the top of the foot ceased to pulsate. "*The perspiration of the foot increased during its application, and a glow was felt* upon its removal. The sensibility gradually returned to the skin over the ankle, the power of motion succeeded, and in about ten weeks the patient was perfectly well."

The author then states, that "another case has since occurred. In four days after the application of the tourniquet the disease began to abate."

The remaining part of the paper is taken up with remarks on the modus operandi of the tourniquet and its effects when applied to one of the extremities.

"Pressure," says Dr. C. "upon the limb acts by suspending its functions for a short time, and thus rendering it more sensible to the operation of the blood, and the influence of the brain." In proof of this doctrine he alleges that, "the sensation and temperature are increased when the pressure is removed." We apprehend, however, that this explanation of the modus operandi of pressure, in augmenting the quantity of blood in a limb, is erroneous, and does not admit of any substantial support. "After removing the tourniquet," says our author "the sensibility was increased, as was evinced by the *pricking sensation* and sense of fulness;" and hence he infers the increased sensibility to the operation of the blood. We know perfectly well, that on removing a ligature from a limb a pricking sensation will ensue; this, however, does not prove that the sensibility is greater than before the application of the pressure. The pricking sensation and fulness are in fact but symptoms of returning sensation, and circulation; for when these again become regularly established the sensation in question vanishes.

In the case above related, the author thinks that the beneficial effects of the pressure resulted from the increased sensibility it created to the action of the blood and brain, and from the consequent greater flow of blood to the foot. We do not doubt that the advantages gained were derived from an increase in the cir-

culation of the part; but we believe that this increase of the quantity of blood in the paralytic foot, was the effect of local congestion, produced by an obstruction to the return of the blood, whilst its access was not wholly prevented. We suspect that the pressure was not so great as to compress the arteries entirely, and that the veins having been entirely obstructed, a local congestion in the foot was the consequence. We cannot concieve how "*the perspiration of the foot*" could have been "increased during the application of the tourniquet," if this was not the case.

That advantages may be gained in paralysis from local congestions, thus produced, is not at all improbable, since the great influence of arterial blood upon the muscular sensibility and irritability, is a fact well established by the researches of modern physiologists.

"*Art. VIII. A Singular Case of Tænia, or Tape-Worm, communicated by* CHARLES CALDWELL, M. D. *Professor of the Institutes of Medicine, &c. in the University of Transylvania.*"

Mr. J. P. had been subject to tænia for three years,—"spirits of turpentine, rust of iron, and various preparations of mercury" had been given with very little advantage. Tin filings, given in large doses, "and followed by active saline purgatives," brought away, at different times, about thirty feet of the worm. At length the following prescription was given "by a physician who, if he had not acquired celebrity for his skill, had at least become noted for the intrepidity of his practice."

℞ Hydrargyri, Stanni } āā ℥xii. f. amalgama.

This was divided into twenty doses, one of which was to be taken at intervals of one hour! The patient, very heroically, we think, began to swallow this amalgam, until he had taken 15 ounces. On the following day he took a saline purgative, which brought away a *portion* of the worm, measuring forty feet. What is particularly singular in this case is, that very little of the amalgam was discharged, and that the "main bulk of it still remains

in his bowels." Shortly afterwards, the patient swallowed four doses more of this amalgam, without however deriving any further advantage from it. Of the whole mass swallowed, he thinks about two ounces have been evacuated; *eighteen* ounces therefore still remain with him. "The metallic mass, of a roundish figure, and in an impacted and firmly contracted and resisting state, is *plainly* and *distinctly* perceptible to the touch." Its size is about that of a pound bullet.

This lump of the amalgam is attached to *one* spot; "but the intestine is perfectly moveable." When the patient leans forward, "it may be readily grasped between the fingers and the thumb. The general health of Mr. P. has as yet not received any obvious shock from this extraneous body lodged in his bowels, although it has already lain there nearly eight months."

"REVIEWS."

"*Art. XII. A new Theory of Galvanism, &c. &c. By* ROBERT HARE, M. D. *Professor of Chemistry in the University of Pennsylvania.*"

This is an able review: we have not room to give an analysis of it. The result however, of the very satisfactory chain of arguments brought forward by the reviewer, in relation to the professor's *theory*, is, "that mechanical and galvanic electricity, are but modifications of the same influence, and consequently, that the theory of Dr. Hare cannot be sustained." With regard to the *calorimotor*, our reviewer entertains the same ideas that have been expressed in this journal. He says, "*It is impossible to read the above description, without being convinced that Dr. Hare's calorimotor is* ESSENTIALLY *the same instrument as the elementary battery of Dr. Wollaston. It is, like that, a voltaic apparatus of* TWO *plates, and it is employed for the same purpose of igniting metals.*"

"*Art. XV. An Inaugural Essay on Mania à Potu. By* ISAAC C. SNOWDEN, M. D. *of Philadelphia.*"

Nosology, or at least common medical language, never at a loss for a name, has in the instance of madness from strong drink, been

particularly liberal. We can at present call to recollection not less than four different names which have been applied to it by different writers. By Dr. Klapp it is called mania à temulentia, an expression derived by him from the nosology of Sauvages. By Dr. Albers of Germany, it is styled delirium tremens; with some others mania à potu is very common, and the late Dr. Rush, from meeting with it frequently among a certain description of shopkeepers, has been heard to call it by an expression about as appropriate as the last—*grocer's mania.*

The reviewer of this essay, states that emetics are only of use in the treatment of mania à potu, by increasing the sensibility to the action of stimulants. "But sensibility being awakened, all is done, (says he) that can be effected by emetics, and we must, to confirm the cure, revert to the stimulating articles, as already described." That this is an unfair statement, is proved by the indubitable fact, of numerous entire cures, even of the most violent cases, having been effected by emetics, without the aid of a drop of laudanum and "hot toddy," or a grain of camphor. Hence it follows, that our reviewer must either admit that he has underrated the extent of power possessed by emetics, or that the accomplishment of cures depends simply "on awakening sensibility." To us it is a matter of indifference, which of the two conclusions be adopted: we shall therefore leave him to choose for himself. His general direction to recur to the use of stimulants, after using emetics, is unnecessary, and perhaps improper; as would appear from the fact above stated. This *mixture* of practice in the ordinary or current cases is useless, and can never be necessary, except in very low cases, and its being found necessary even then, is a proof of the importance of emetics. However safe and correct our reviewer may be in making the assertion, that "to produce sleep is the grand point to which the practitioner must steadily direct his attention," as regards those extremely low cases of the disease, in which life appears to be involved in immediate danger, yet we feel ourselves authorised to declare, both from what we have seen, and what we have read upon this subject, that in the more ordinary cases, the attention should be directed to a previous and more important indication of cure. Having had no

reason to alter our opinion already before the medical public, of the gastric origin of the disease in question, we therefore, in this place do not hesitate to affirm, that the indication of cure is not what the reviewer asserts: viz. to induce sleep, but to *evacuate and change the condition of the stomach.* This being effected, we trust it will be found, on the principle of *sublata causa*, &c. that sleep will spontaneously follow. We derive great support in this view of the subject from the fact, that in many instances it has been remarked, that the operation of the emetic is generally soon followed by profound and refreshing sleep, even in cases where the most potent anodynes had already been unavailingly administered. We therefore repeat the assertion, that in every case of mania à potu, where the attendant debility is not too great to bear the emetic, the true and appropriate indication is to produce vomiting. Between this mode of practice and that of the opiates and stimulant treatment, we conceive there exists all the difference that there is between prescribing for the removal of the causes and the symptoms of diseases. Waving, however, any further advantages in the discussion, deducible from allusions to the pathology of the disease, which some of our readers may think still admits of a degree of uncertainty, we shall contend for the superiority of Dr. Klapp's mode of treating this disease, from its effecting a greater number of cures; and all must admit that this is the best, and the only substantial basis on which the claims of preference could be fixed.

We recollect not long since to have been present when a medical gentleman of eminence, and a colleague of Dr. Klapp's, publicly asserted in the Medical Society of this city, that judging from his own experience the emetic practice was decidedly superior, to every other method, and that since its introduction into the Alms-house infirmary, a death from mania à potu had become a rare occurrence. Unquestionably, in that very respectable institution, the best opportunities are afforded to decide the respective claims of the two modes of practice to preference. There are, we believe, more admissions of this disease into the Alms-house, than into all the other medical establishments of this city put together. The resident pupils of the house, have also

given very decided, and indeed very honourable evidence of the superority of the emetic treatment of the disease in question. Several of them have considered this practice of sufficient importance to be made the subject of their inaugural dissertations.

The facility with which cures are performed by the new practice, forms one of its distinguished traits. We are credibly informed that at the Alms-house, instances have occurred of patients being admitted on one day raving, and discharged, or at least cured against the next. We also have had experience of its prompt efficacy, and feel as if we had a right to speak of it with some practical authority.*

The reviewer refers to unsuccessful cases; in reply to which, he must be reminded that all diseases have their irremediable cases, and that the new practice, though its advantages are great, has not yet been recommended as infallible. No doubt when large doses of tartar emetic have been indiscretely persevered in, mischief has been done. Dr. Klapp, however, is not to be censured for this, for in his earliest publication of this practice, he has not only intimated the danger, but has supplied a remedy for the difficiency of the antimony. On this subject see the second number of the sixth volume of the Eclectic Repertory, where Dr. K. expresses himself in the following manner. "In relation, however, to this method of treatment, I think it necessary to state, that in this disease, the stomach has usually proved to be in *a very torpid insensible condition*, and in some instances this has been the case to such a degree as to require a *scruple* and even *twenty-eight grains* of the tartarized antimony to be administered, before puking could be excited. I have also found, when the stomach could not be excited by vomiting by antimony, or by such doses of it as were *deemed safe to give*, that this effect has been happily produced by changing the article for *ipecacuanha*, but not until very large doses of the latter had been administered."

We entirely concur with the author of the review in one point of weighty consideration, that it is "the imperative and sacred duty of the physician to unite and confederate with the divine, the moralist, the economist, and all other good men, to restrain

* See a case, in vol. I. page 180, of this journal.

the consumption, of those baneful articles," rum, brandy, gin and whiskey, " and as the first step in the scheme of reformation, to discountenance by the weight of his authority, the popular notion of their remedial efficacy, and to seize every fit opportunity, to state, truely and earnestly their pestilent operation."

We must beg leave however, to arrest for a moment the soarings of his pen, and ask him seriously, whether the mode of practice he recommends, " for one of the infinity of the afflictive consequences of the odious and detestable practice of hard drinking," is not in some respects calculated to disseminate the very evil for the extirpation of which, he advises the physician, to confederate with all good men. The best authors, which we have looked into, on this subject, most strenuously advise habitual inebriates in particular, wholly and perseveringly, to avoid every species of ardent, and vinous drink, as the best, and only true way of emancipating themselves from that most disgusting practice. " *Touch not, taste not,*" was the emphatic admonition of one of the greatest medical *professors* of the age; a most inestimable maxim, the sound of which, it is presumable, still hangs on the ears of hundreds of the medical profession in this country, and it were well for humanity, if every individual of the human family had equally participated in the salutary effects of the warning voice. Would it not be natural to suppose that the remedies advised by the reviewer, would tend to render more general the practice of *drinking*, when the first and the last prescription for him who has already become mad from drunkenness " *is the brandy treatment.*" On his being received into the cell of an infirmary, to drench him with whiskey and then to discharge him from the institution cured, but with the pestiferous cup in his hand, has according to our apprehension, very little of the appearance of the physician confederating with either " the divine or the economist" to restrain the consumption of ardent liquors.

Art. XXI. *Literary Intelligence.*

Dr. Goodman of Baltimore is engaged in preparing a translation of the celebrated work of Dr. Gregory, of Edinburgh, on the Principles of Medicine, to which will be added the latest discoveries of the European and American physiologists. This work is expected to appear by the month of May ensuing.

Just published, at Boston, *Medical dissertation on Hæmoptysis and on suppuration*, which obtained the Boyleston premium, for the years 1818 and 1820. By John Ware, M. D. &c.

We have not yet been able to obtain a copy of this book, but shall endeavour to give our readers some account of it, in our next number. From the interesting nature of the topics and our knowledge of the talents of this gentleman, we have no doubt it will be found interesting.

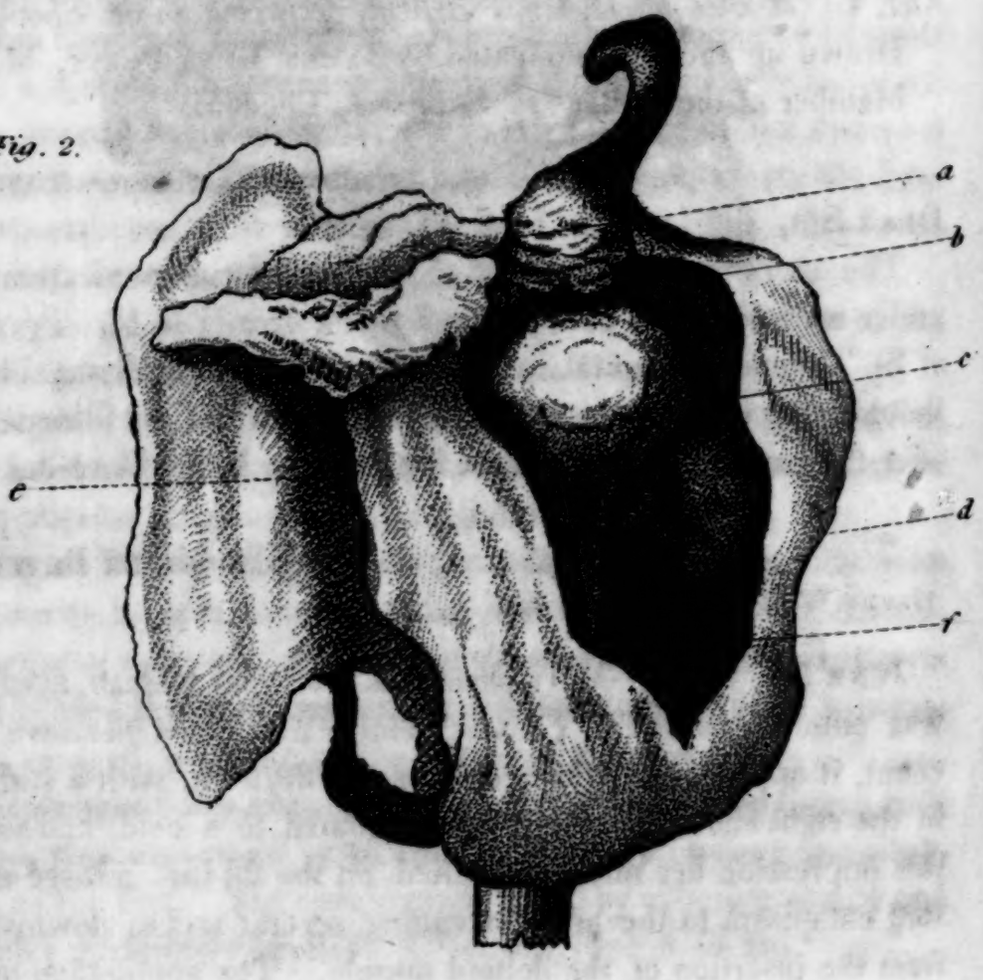
UNIVERSITY OF PENNSYLVANIA.

Dr. Horner, of this city, has been elected Adjunct Professor of Anatomy in the University of Pennsylvania.

Erratum. For *lumbrici teres* in page 491 of our last number read *lumbrici teretes.*

Fig. 1.

Fig. 2.



HYDROPS ARTICULI.

Fourth Vol: Am: Med: Rec: page 209.